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ARTICLES

AN EVOLUTIONARY APPROACH TO NORMS

ROBERT AXELROD
University of Michigan

Norms provide a powerful mechanism for regulating conflict in groups, even when there are more than two people and no central authority. This paper investigates the emergence and stability of behavioral norms in the context of a game played by people of limited rationality. The dynamics of this new norms game are analyzed with a computer simulation based upon the evolutionary principle that strategies shown to be relatively effective will be used more in the future than less effective strategies. The results show the conditions under which norms can evolve and prove stable. One interesting possibility is the employment of metanorms, *the willingness to punish someone who did not enforce a norm. Many historical examples of domestic and international norms are used to illustrate the wide variety of mechanisms that can support norms, including metanorms, dominance, internalization, deterrence, social proof, membership in groups, law, and reputation.*

An established norm can have tremendous power. This is illustrated by a historical instance of the norm of dueling. In 1804 Aaron Burr challenged Alexander Hamilton to a duel. Hamilton sat down the night before the duel was to take place and wrote down his thoughts. He gave five reasons against accepting the duel: his principles were against shedding blood in a private combat forbidden by law; he had a wife and children; he felt a sense of obligation toward his creditors; he bore no ill against Colonel Burr; and he would hazard much and could gain little. Moreover, he was reluctant to set a bad example by accepting a duel. Yet he did accept, because "the ability to be useful, whether in resisting mischief or effecting good, in those crises of our public affairs which seem likely to happen, would probably be inseparable from a conformity with public prejudice in this particular" (Truman, 1884, pp. 345–48). In other words, the prospect of sanctions imposed by the general public in support of dueling caused Hamilton to risk, and ultimately to lose, his life—a powerful norm indeed, and yet one that has all but disappeared today after centuries of power over life and death.

Today, norms still govern much of our political and social lives. In politics, civil rights and civil liberties are as much protected by informal norms for what is acceptable as they are by the powers of the formal legal system. Leadership is itself subject to the power of norms, as Nixon learned when he violated political norms in trying to cover up Watergate. The operation of Congress is shaped by many norms, including those governing reciprocity (Matthews, 1960) and apprenticeship (Krehbiel, 1985). Across many nations, tolerance of opposition is a fragile norm that has great impact on whether a democracy can survive in a given country (Almond and Verba, 1963; Dahl, 1966). In international political economy, norms are essential for the understanding of the operations of many

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functional domains such as banking, oil, and foreign aid (Axelrod and Keohane, 1985; Keohane, 1984; Krasner, 1983). Even in the domain of power politics, norms have virtually wiped out colonialism, inhibited the use of chemical warfare, and retarded the spread of nuclear weapons.

Not only are norms important for many central issues in political science, but they are vital to the other social sciences as well. Sociology seeks to understand how different societies work, and clearly norms are important in these processes (e.g., Opp, 1979, 1983). Anthropology frequently deals with the unique features of various peoples by describing in great detail their practices and values, as in the case of feuding (e.g., Black-Michaud, 1975). Psychologists are concerned with how people influence each other and the manner in which an individual becomes socialized into a community (e.g., Darley and Batson, 1973; Sherif, 1936). Economists are becoming interested in the origin and operation of norms as they have come to realize that markets involve a great deal of behavior based on standards that no one individual can determine alone (e.g., Furubotn and Pejovich, 1974; Schotter, 1981).

Large numbers of individuals and even nations often display a great degree of coordinated behavior that serves to regulate conflict. When this coordinated behavior takes place without the intervention of a central authority to police the behavior, we tend to attribute the coordinated behavior and the resulting regulation of conflict to the existence of norms. To make this appeal to norms a useful explanation, we need a good theory of norms. Such a theory should help explain three things: how norms arise, how norms are maintained, and how one norm displaces another.

One of the most important features of norms is that the standing of a norm can change in a surprisingly short time. For example, after many centuries of colonialism, the intolerance of colonial dependence took hold in the relatively short period of just two decades after World War II. Before and after such a transition, the state of affairs seems very stable and perhaps even permanent. For this reason, awareness of a given norm is most intense precisely when it is being challenged. Examples of norms being challenged today include the right to smoke in public without asking permission, the use of gender-laden language, and the prohibition against the use of chemical warfare. Some of these challenges will succeed in establishing new norms, and some will fail altogether. Thus, what is needed is a theory that accounts not only for the norms existing at any point in time, but also for how norms change over time. To clarify these processes, one must first be clear about exactly what is being discussed.

In this next section the evolutionary approach to be used in this paper is explained. Following this, the results of computer simulations of the evolution of norms are presented. The computer simulations are then extended to include a specific mechanism for the enforcement of norms, called *metanorms.* After these formal models are investigated, a wide variety of processes that might help to sustain norms are discussed, along with suggestions about how they too can be modeled. The question of the origin and content of norms is considered, and finally, a summary and conclusion presents the findings of this paper in the broad context of social and political change.

The Evolutionary Approach

Norms have been defined in various ways in the different literatures and even within the same literature. The three most common types of definitions are based upon expectations, values, and behavior.

That these different definitions are used for the same concept reflects how expectations, values, and behavior are often closely linked. Definitions based upon expectations or values are favored by those who study norms as they exist in a given social setting. Such definitions are convenient because interviews can elicit the beliefs and values of the participants, whereas systematically observing their actual behavior is more difficult. Because for many purposes the most important thing is actual behavior, a behavioral definition will be used in this study.

DEFINITION. *A* norm *exists in a given social setting to the extent that individuals usually act in a certain way and are often punished when seen not to be acting in this way.*

This definition makes the existence of a norm a matter of degree, rather than an all or nothing proposition, which allows one to speak of the growth or decay of a norm. According to this definition, the extent to which a given type of action is a norm depends on just how often the action is taken and just how often someone is punished for not taking it.

To investigate the growth and decay of norms, I have formulated a norms game in which players can choose to defect and to punish those they have seen defecting. The goal of the investigation is to see when cooperation based upon emerging norms will develop. Ultimately, the purpose is to learn what conditions favor the development of norms so that cooperation can be promoted where it might not otherwise exist or be secure.

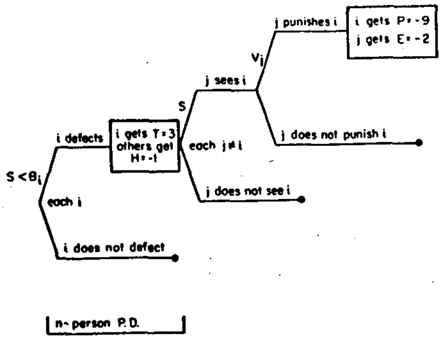
To see what rational actors would do in a particular setting, a game theory approach can be used. Game theory assumes the players are fully rational and choose the strategy that gives the highest expected utility over time, given their expectations about what the other players will do. Recent work by economists has shown great sophistication in dealing with problems of defining credible threats and of showing the consequences of requiring actors' expectations about each other to be consistent with the experience that will be generated by the resulting actions (Abreu, Pearce, and Stacchetti, 1985; Friedman, 1971; Kreps and Wilson, 1982; Selten, 1975).

While deductions about what fully rational actors will do are valuable for their own sake, empirical examples of changing norms suggest that real people are more likely to use trial and error behavior than detailed calculations based on accurate beliefs about the future. Therefore, I have chosen not to study the dynamics of norms using an approach that depends on the assumption of rationality.

Instead, I use an evolutionary approach. This approach is based on the principle that what works well for a player is more likely to be used again while what turns out poorly is more likely to be discarded (Axelrod, 1984). As in game theory, the players use their strategies with each other to achieve a payoff based upon their own choice and the choices of others. In an evolutionary approach, however, there is no need to assume a rational calculation to identify the best strategy. Instead, the analysis of what is chosen at any specific time is based upon an operationalization of the idea that effective strategies are more likely to be retained than ineffective strategies. Moreover, the evolutionary approach allows the introduction of new strategies as occasional random mutations of old strategies.

The evolutionary principle itself can be thought of as the consequence of any one of three different mechanisms. It could be that the more effective individuals are more likely to survive and reproduce. This is true in biological systems and in some economic and political systems. A second interpretation is that the players learn by trial and error, keeping effective strategies and altering ones that turn out

Figure 1. Norms Game



Key:

i, j	individuals
S	probability of a defection being seen by any given individual
B_i	boldness of i
V_j	vengefulness of j
T	player's temptation to defect
H	hurt suffered by others
P	cost of being punished
E	enforcement cost

poorly. A third interpretation, and the one most congenial to the study of norms, is that the players observe each other, and those with poor performance tend to imitate the strategies of those they see doing better. In any case, there is no need to assume that the individual is rational and understands the full strategic implications of the situation.

The evolutionary approach is inherently probabilistic and involves nonlinear effects. For these reasons, it is often impossible to use deductive mathematics to determine the consequences of a given model. Fortunately, computer simulation techniques (e.g., Cyert and March, 1963) provide a rigorous alternative to deductive mathematics. Moreover, simulation can reveal the dynamics of a process, as well as the equilibrium points. By simulating the choices of each member of a population of players and by seeing how the players' strategies change over time, the unfolding of a given evolutionary process can be analyzed to determine its overall implications.

The Norms Game

The norms game is described in Figure 1. It begins when an individual (i) has an opportunity to defect, say by cheating on an exam. This opportunity is accompanied by a known chance of being observed. The chance of being observed, or *seen*, is called S. If S is .5, each of the other players has an even chance of observing a defection if it takes place. If player i does defect, he or she gets a payoff of T (the *temptation* for defecting) equal to 3, and each of the others are *hurt* (H) slightly, getting a payoff of H equal to -1. If the player does not defect, no one gets anything.

So far the game is similar to an n-person Prisoner's Dilemma (see, e.g., G. Hardin, 1968; R. Hardin, 1982; Schelling, 1978). The new feature comes in the next step. If player i does defect, some of the other players may see the defection, and those who do may choose to punish the defector. If the defector is *punished* (P) the payoff is a very painful $P = -9$, but because the act of punishment is typically somewhat costly, the punisher has to pay an *enforcement cost* (E) equal to -2.

The strategy of a player thus has two dimensions. The first dimension of player i's strategy is *boldness* (B_i), which determines when the player will defect. The player will defect whenever the chance of being seen by someone is less than the player's boldness, which is to say, whenever $S < B_i$. The second dimension of a player's strategy is *vengefulness* (V_i), which is the probability that the player will punish someone who is defecting. The greater the player's vengefulness, the more likely he or she will be to punish someone who is spotted defecting.

Table 1. Example of Payoffs in the Norms Game Attained by a Player With Boldness Equal to 2/7 and Vengefulness Equal to 4/7

Event	Payoff per Event	Number of Events	Payoff
Defection	$T = 3$	1	3
Punishment	$P = -9$	1	-9
Hurt by others	$H = -1$	36	-36
Enforcement cost	$E = -2$	9	-18
Score			-60

Simulation of the Norms Game

The simulation of the norms game determines how the players' strategies evolve over time. The two dimensions of a strategy, boldness and vengefulness, are each allowed to take one of eight levels, from 0/7 to 7/7. Because the representation of eight levels requires three binary bits, the representation of a player's strategy requires a total of six bits, three for boldness and three for vengefulness.

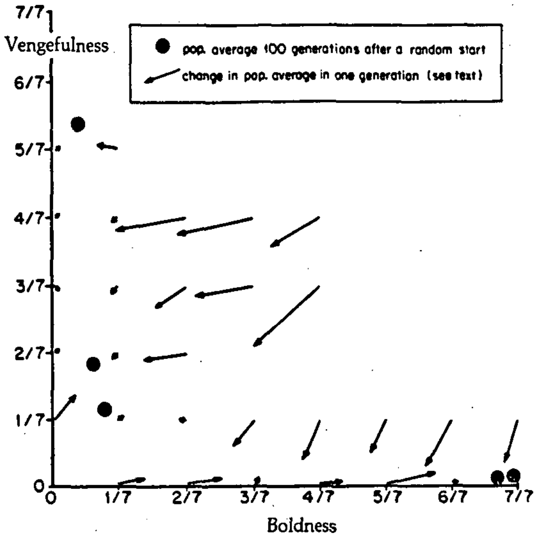
The simulation itself proceeds in five steps, as follows:

(*1*) The strategies for the initial population of 20 players are chosen at random from the set of all possible strategies.

(*2*) The score of each player is determined from the player's own choices and the choices of the other players. Each individual gets four opportunities to defect. For each of these opportunities, the chance of being seen, S, is drawn from a uniform distribution between 0 and 1. To see how the scores are attained, let us focus on an arbitrary player in the initial population of one of the runs, who will be called Lee. Lee has a boldness level of 2/7 and vengefulness level of 4/7. The total payoff Lee achieved was the result of four different kinds of events, as shown in Table 1. Lee defected only once because only one of the four opportunities had a chance of being seen that was less than Lee's boldness of 2/7. This defection gave a temptation payoff of $T = 3$ points. Unfortunately for Lee, one of the other players observed the defection and chose to punish it, leading to a loss for Lee of $P = -9$ points. In addition the other players defected a total of 36 times, each hurting Lee $H = -1$ point. Finally, Lee observed who was responsible for about half of these defections and chose to punish each of them with a probability determined by his vengefulness of 4/7. This lead to a punishment of 9 of the defections at an enforcement cost of $E = -2$ each, for a further loss of 18 points. The net result of these four types of events was a total score of -60 for Lee.

(*3*) When the scores of all the players are determined, individuals whose strategies were relatively successful are selected to have more offspring.[1] The method is to give an average individual one offspring and to give two offspring to an individual who is one standard deviation more effective than the average. An individual who is one standard deviation below the population average will not have his or her strategy reproduced at all. For convenience, the number of offspring is adjusted to maintain a constant population of 20. A final step is the introduction of some mutation so that new strategies can arise and be tested. This is done by allowing a 1% chance that each bit of an individual's new strategy will be altered. This mutation rate gives a little more than one mutation per generation in the entire population.

Figure 2. Norms Game Dynamics



(4) Steps 2 and 3 are repeated for 100 generations to determine how the population evolves.

(5) Steps 1 to 4 are repeated to give five complete runs of the simulation.

The results of the five runs are shown in Figure 2. The five circles indicate the average boldness and vengefulness of a population after 100 generations. Three completely different outcomes appear possible. In one of the runs, there was a moderate level of vengefulness and almost no boldness, indicating the partial establishment of a norm against defection. On two other runs there was little boldness and little vengefulness, and on the remaining two runs, there was a great deal of boldness and almost no vengefulness—the very opposite of a norm against defection. What could be happening?

The way the strategies actually evolve over time is revealed by the change that takes place in a single generation in a population's average boldness and vengefulness. To calculate this, the data are used from all 100 generations of all five runs, giving 500 populations. The populations with similar average boldness and vengefulness are then grouped together, and their average boldness and vengefulness one generation later is measured. The results are indicated by the arrows in Figure 2.

Now the various outcomes begin to fit into a common pattern. All five of the runs begin near the middle of the field, with average boldness and vengefulness levels near one-half. The first thing to happen is a dramatic fall in the boldness level. The reason for the decline is that when there is enough vengefulness in the population, it is very costly to be bold. Once the boldness level falls, the main trend is a lowering of vengefulness. The reason for this is that to be vengeful and punish an observed defection requires paying an enforcement cost without any direct return to the individual. Finally, once the vengefulness level has fallen nearly to zero, the players can be bold with impunity. This results in an increase in boldness, destroying whatever restraint was established in the first stage of the process—a sad but stable state in this norms game.

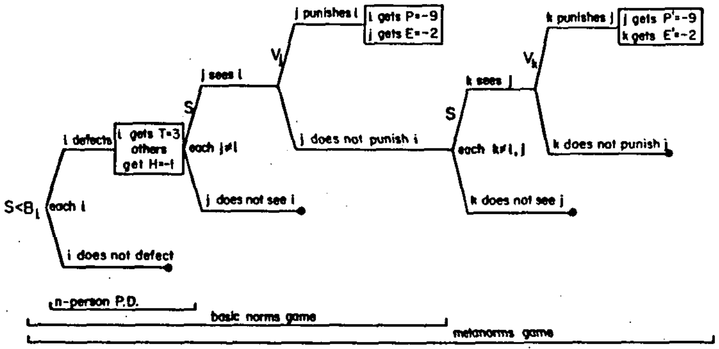
This result raises the question of just what it takes to get a norm established. Because the problem is that no one has any incentive to punish a defection, the next section explores one of the mechanisms that provides an incentive to be vengeful.

Metanorms

A little-lamented norm of once great strength was the practice of lynching to enforce white rule in the South. A particularly illuminating episode took place in Texas in 1930 after a black man was arrested for attacking a white woman. The mob was impatient, so they burned down the courthouse to kill the prisoner within. A witness said,

> I heard a man right behind me remark of the fire, "Now ain't that a shame?" No sooner had the

Figure 3. Metanorms Game


j punishes i
i gets P=-9
j gets E=-2
V_j
j sees i
S
i defects
i gets T=3 others get H=-1
each j≠i
j does not punish i
j does not see i
S<B_i
each i
i does not defect
k punishes j
j gets P'=-9
k gets E'=-2
V_k
k sees j
S
each k≠i,j
k does not punish j
k does not see j
n-person P.D.
basic norms game
metanorms game

Key:

i,j,k	individuals
S	probability of a defection's being seen by any given individual
B_i	boldness of *i*
V_j	vengefulness of *j*
T	temptation to defect
H	hurt suffered by others
P	cost of being punished
E	punisher's enforcement cost
P'	cost of being punished for not punishing a defection
E'	cost of punishing someone for not punishing a defection

> words left his mouth than someone knocked him down with a pop bottle. He was hit in the mouth and had several teeth broken. (Cantril, 1941, p. 101)

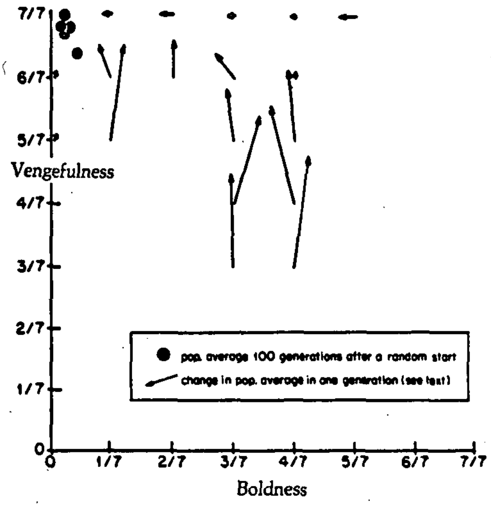
This is one way to enforce a norm: punish those who do not support it. In other words, be vengeful, not only against the violators of the norm, but also against anyone who refuses to punish the defectors. This amounts to establishing a norm that one must punish those who do not punish a defection. This is what I will call a *metanorm.*

Metanorms are widely used in the systems of denunciation in communist societies. When the authorities accuse someone of doing something wrong, others are called upon to denounce the accused. Not to join in this form of punishment is itself taken as a defection against the group (Bronfenbrenner, 1970; Meyers and Bradbury, 1968).

As another example, when the Soviet Union supported the suppressionn of the Solidarity movement in Poland, the United States asked its allies to stop supplying components to the Soviet Union for its new gas pipeline. The allies, not wanting to pay the enforcement cost of this punishment, refused. The United States government then undertook the metapunishment of imposing sanctions on foreign companies that defied the sales ban (*New York Times,* January 5 and June 19, 1982).

The formulation of a metanorms game can help in the exploration of the effectiveness of this mechanism. Figure 3 shows how the metanorms game is based upon an extension of the norms game. If

Figure 4. Metanorms Game Dynamics



someone defects, and Lee sees but does not punish that defection, then the other players have a chance to see and punish Lee. The model makes the critical assumption that a player's vengefulness against nonpunishment is the same as the player's vengefulness against an original defection.[2] The validity of this assumption will be addressed later, but first let us see what affect it has on the evolution of the process.

A set of five runs was conducted with the metanorms game, each done as before with a population of 20 players and a duration of 100 generations. The results are shown in Figure 4. They are unambiguous. In all five runs a norm against defection was established. The dynamics are clear. The amount of vengefulness quickly increased to very high levels, and this in turn drove down boldness. The logic is also clear. At first there was a moderate amount of vengefulness in the population. This meant that a player had a strong incentive to be vengeful, namely, to escape punishment for not punishing an observed defection. Moreover, when each of the players is vengeful out of self-protection, it does not pay for anyone to be bold. Thus the entire system is self-policing, and the norm becomes well established.

This result is dependent, however, on the population's starting with a sufficiently high level of vengefulness. Otherwise the norm still collapses. Thus, while the norms game collapses no matter what the initial conditions are, the metanorms game can prevent defections if the initial conditions are favorable enough.

Mechanisms to Support Norms

The simulations of the norms game and the metanorms game have allowed the exploration of some of the important processes in the dynamics of norms. The simulation of the norms game shows that relying on individuals to punish defections may not be enough to maintain a norm. Therefore, the question to be considered now is, What mechanisms can serve to support a norm that is only partially established? The evolutionary approach helps to develop a list of such processes, and in some cases, suggests specific methods for modeling the process by which a norm can be supported.

Metanorms

As the computer simulations show, the existence of a metanorm can be an effective way to get a norm started and to protect it once it is established. By linking vengefulness against nonpunishers with vengefulness against defectors, the metanorm provides a mechanism by which the norm against defection becomes self-policing. The trick, of course, is to link the two kinds of vengefulness. Without this link, the system could unravel. An individual might reduce the metavengeance level while still being vengeful and then later stop being vengeful when others stopped being metavengeful.

The examples cited earlier suggest that

people may well punish those who do not help to enforce a valued norm. The model suggests norms can be supported if people tend to have correlated degrees of vengefulness or anger against someone who violates a particular norm and someone who tolerates such a violation. What the evolutionary approach has done is raise the possibility that metanorms are a mechanism that can help support norms, thus suggesting the interesting empirical question of whether the two types of vengefulness are indeed correlated. My guess is that there is such a correlation. The types of defection we are most angry about are likely to be the ones whose toleration also makes us angry. As of now, however, the possibility of metanorms remains speculative.

Dominance

Another mechanism for supporting a norm is the dominance of one group over another. For example, it is no coincidence that in the South, whites lynched blacks, but blacks did not lynch whites. The whites had two basic advantages: greater economic and political power, and greater numbers.

Simulation of the effects of power and numbers can be readily done with slight extensions of the basic model to allow for the existence of two different groups. The competition between two groups can be modeled by assuming that the defections of a player only hurt the members of the other group and are therefore only punished by members of the other group. Similarly, in the metanorms version of the model, punishments for not punishing a defector would only occur within a group, as illustrated by the pop bottle used by one white against another in the lynching example discussed above. Moreover, in determining strategies for the next generation, the strategies of two groups would be allowed to adapt separately so that whites learn from whites and blacks learn from blacks.

The two advantages of the whites are modeled separately. Their greater economic or political power is reflected in their lessened cost of being punished by a black. This was done by letting $P = -3$ for whites while retaining $P = -9$ for blacks. The greater numbers of whites are reflected directly in the relative size of the two populations, giving the whites a greater chance to observe and punish a black defection than vice versa. This was done by letting the population be 20 whites and 10 blacks.

Analysis of runs based upon these conditions shows that resistance to punishment and increased size can help a group, but only if there are metanorms. Without metanorms, even members of the stronger group tend to be free riders, with no private incentive to bear enforcement costs. This in turn leads to low vengefulness and high boldness in both groups. When metanorms are added, it becomes relatively easier for the strong group to keep the weak group from being bold, while it is not so easy for the weak group to keep the strong one from defecting.

Another form of potential strength is illustrated by the case of a major power interacting with many smaller nations. For example, the U.S. may not only be in a favorable position on a given bilateral interaction but also may have many more bilateral interactions than others. Thus, its behavior has a greater impact on the development of norms than would the behavior of a minor power. When Libya wanted to modify the international norm of the twelve-mile limit of territorial waters to include the entire Gulf of Sidra, the United States fleet deliberately sailed into the Gulf and shot down two Libyan planes sent up to try to change the norm. Clearly the U.S. was not only stronger but had incentives to enforce the old norm based upon its naval interests in other parts of the world.

While the process of frequent interactions by a single strong player has not yet been simulated, it is plausible that such a process would help to establish a norm against defection because the central player would have a greater unilateral incentive to be vengeful against defections.

Norms can also be promoted by the interests of a few major actors, such as the U.S. and the Soviet Union's both working to retard the proliferation of nuclear weapons. Their actions need not be coordinated in detail as long as together they are important enough to others to enforce a norm of the major actors' choice. The logic is somewhat analogous to Olson's "privileged group" in a collective action problem (Olson, 1965, pp. 48–50).

Internalization

Norms frequently become internalized (Scott, 1971). This means that violating an established norm is psychologically painful even if the direct material benefits are positive. This is frequently observed in laboratory experiments where subjects are more equitable than they have to be and explain their behavior by saying things like "you have to live with yourself." In terms of the norms game, this type of internalization means that the temptation to defect, *T*, is negative rather than positive. If everyone internalizes a given norm this strongly, there is no incentive to defect and the norm remains stable. Obviously families and societies work very hard to internalize a wide variety of norms, especially in the impressionable young. They do so with greater or lesser success depending on many factors, including the degree to which the individual identifies with the group and the degree to which the norm and its sponsors are seen as legitimate.[3]

Clearly, it is rare for everyone in a group to have a norm so strongly internalized that for each the temptation to defect is actually negative. An interesting question for future modeling is, How many people have to internalize a norm in order for it to remain stable?

The logic of the norms game suggests that lowering the temptation to defect might not be enough. After all, even if most people did not defect, if no one had an incentive to punish the remaining defectors, the norm could still collapse. This point suggests that we look for internalization, not only in the reduced incentive to defect, but also in an increased incentive to punish someone else who does defect.

An increased incentive to punish, through internalization or by some other means, would lead some people to feel a gain from punishing a defector. For them, the payoff from enforcement, *E*, would actually be positive. Such people are often known as self-righteous busy bodies and often are not very well liked by those who enjoy a defection now and then. Given enough people who enjoy enforcing the norm, the question of its maintenance then becomes whether the chance is high or low that the defection will be seen.

Deterrence

In the norms game and the metanorms game the players do not look ahead. Instead they try a particular strategy, see how it does, compare their payoff with the payoff of others, and switch strategies if they are doing relatively poorly. While trial and error is a sensible way of modeling players of very limited rationality, it does not capture the idea that players may have a great enough understanding of the situation to do some forward-looking calculations as well as backward-looking comparisons with others. In particular, a person may realize that even if punishing a defection is costly now, it might have long-term gains by discouraging other defections later.

A good example is the strong U.S. response to New Zealand's refusal in February 1985 to allow a U.S. destroyer into

Aukland harbor without assurances that it did not carry nuclear weapons. The U.S. government presumably did not care very much about nuclear access to New Zealand ports, but it did care a great deal about deterring the spread of a new norm of "nuclear allergy" among its many allies in other parts of the world (Arkin and Fieldhouse, 1985).

Social Proof

An important principle from social psychology is "social proof," which applies especially to what people decide is correct behavior. As Cialdini (1984, p. 117) explains,

> we view a behavior as more correct in a given situation to the degree that we see others performing it. Whether the question is what to do with an empty popcorn box in a movie theater, how fast to drive on a certain stretch of highway, or how to eat chicken at a dinner party, the actions of those around us will be important in defining the answer.

The actions of those around us serve several functions. First, they provide information about the boldness levels of others, and indirectly about the vengefulness of the population. Hence, we can infer something about whether it pays for us to be bold or not. Second, the actions of others might contain clues about what is the best course of action even if there is no vengefulness. For example, people may be driving slowly on a certain stretch of highway, not because there is a speed trap there, but because the road is poorly paved just ahead. Either way, the actions of others can provide information about how the population has been adapting to a particular environment. If we are new to that environment, this is valuable information about what our own behavior should be (Asch, 1951, 1956; Sherif, 1936). The actions of others provide information about what is proper for us, even if we do not know the reasons. Finally, in many cases, by conforming to the actions of those around us, we fulfill a psychological need to be part of a group.

Our propensity to act on the principle of social proof is a major mechanism in the support of norms. The current model of norms already has a form of this mechanism built in: when a relatively unsuccessful individual seeks a new strategy, that strategy is selected from those being used by the rest of the population. This is a form of social proof, refined by giving weight to the more successful strategies being employed in the population.

In cases where other people differ in important ways, the principle of social proof tends to apply to those who are most like us. This too is easy to build into simulations with more than one group. In the simulation of blacks and whites, the blacks look only to other blacks when selecting a new strategy, and the whites look only to other whites. This makes good sense because a strategy that is very successful for a white might be disastrous if employed by a black.

Membership

Another mechanism for the support of norms is voluntary membership in a group working together for a common end.[4] Contracts, treaties, alliances, and memberships in social groups all carry with them some power to impose obligations upon individuals. The power of the membership works in three ways. First, it directly affects the individual's utility function, making a defection less attractive because to defect against a voluntarily accepted commitment would tend to lower one's self-esteem. Second, group membership allows like-minded people to interact with each other, and this self-selection tends to make it much easier for the members to enforce the norm implicit in the agreement to form or join a group. Finally, the very agreement to form a group helps define what is expected of the participants, thereby clarifying when a defection occurs and when a punishment is called for.

One might suppose it would be easy for a bold individual to join and then exploit a group that had gathered together in the expectation of mutual compliance. Actually, this does not usually happen, in part because the factors just outlined tend to isolate a defector and make it relatively easy for the others to be vengeful—especially with the help of metanorms. Another factor is that, according to recent experimental evidence, cooperators are more likely to stay in a group than are defectors (Orbell, Schwarz-Shea, and Simmons, 1984). This happens because cooperators have a stronger ethical or group-regarding impulse than defectors, a factor that led them to cooperate in the first place.

The metanorms game can be expanded to include the choice of whether to join a group or not.[5] In general, the value to a person of joining a group would depend on how many others joined. Each player would make this choice at the start of the game. Then the interactions concerning defections and punishments would occur as before, with the interactions limited to those who had actually joined. As an example, an alliance for collective security would include a group of nations that had joined for this common purpose. Once a nation had joined, a defection would consist of not supporting the alliance in some collective security task. A defection would hurt the other members of the alliance, and some of them might choose to punish the defector; they might also choose to punish someone who did not punish the defector. Typically, the larger the number of nations joining the group, the greater the benefits of cooperation would be for its members.

In the political sphere, voluntary membership taking the form of a social contract has been a powerful image for the support of democratic forms of governance. In effect, a mythical agreement is used to give legitimacy to a very real set of laws and institutions.

Law

Norms often precede laws but are then supported, maintained, and extended by laws. For example, social norms about smoking in public are now changing. As more and more people turn vengeful against someone who lights up in a confined space, fewer and fewer smokers are so bold as to do so without asking permission. As this norm becomes firmer, there is growing support to formalize it through the promulgation of laws defining where smoking is and is not permitted.[6]

A law supports a norm in several ways. The most obvious is that it supplements private enforcement mechanisms with the strength of the state. Because enforcement can be expensive for the individual, this can be a tremendous asset. In effect, under the law the collective goods problem of enforcement is avoided because selective incentives are given to specialized individuals (inspectors, police, judges, etc.) to find and punish violations.

The law also has a substantial power of its own, quite apart from whether it is or can be enforced. Many people are likely to take seriously the idea that a specific act is mandated by the law, whether it is a requirement to use seat belts or an income tax on capital gains. However, we all know this respect for the law has its limits, and we suspect that many people do not pay all the tax they should. Even when enforcement is possible and is attempted, the strength of the law is limited. In most cases, the law can only work as a supplement (and not a replacement) for informal enforcement of the norm. The failure of Prohibition is a classic example of an attempt to enforce a norm without sufficient social support.

In addition to enforcement and respect, a third advantage of the law is clarity. The law tends to define obligations much more clearly than does an informal norm. A social norm might say that a landlord should provide safe housing for tenants, but a housing code is more likely to define safety in terms of fire escapes. Over the domain covered by the law, the norm might become quite clear. However, this clarity is gained at the expense of suggesting that conformity with the law is the limit of one's social obligations.

Modeling the power and operation of the law is beyond the scope of this project. However, it should still be emphasized that often law is the formalization of what has already attained strength as a social or political norm. An important example is civil liberties, the very foundation of a democratic system. There are laws and constitutional provisions in support of civil liberties such as freedom of speech, but the legal system can only protect free speech if there is substantial support for it among a population willing to tolerate dissent and willing to protect those who exercise it.

In short, social norms and laws are often mutually supporting. This is true because social norms can become formalized into laws and because laws provide external validation of norms. They are also mutually supporting because they have complementary strengths and weaknesses. Social norms are often best at preventing numerous small defections where the cost of enforcement is low. Laws, on the other hand, often function best to prevent rare but large defections because substantial resources are available for enforcement.

Reputation

An important, and often dominant, reason to respect a norm is that violating it would provide a signal about the type of person you are. For example, if there is a norm dictating that people should dress formally for dinner, and you don't, then others might make some quite general inferences about you.

The importance of dressing formally when the occasion requires is not just that others will punish you for violating the norm (say, by giving you a disapproving look) but also that they will infer things about you and then act in ways you wish they wouldn't. This is an example of the signaling principle: a violation of a norm is not only a bit of behavior having a payoff for the defector and for others; it is also a signal that contains information about the future behavior of the defector in a wide variety of situations.[7]

There are several important implications of the signaling principle for the origin and durability of a norm. A norm is likely to originate in a type of behavior that signals things about individuals that will lead others to reward them. For example, if a certain accent signals good breeding, then others may give better treatment to those who speak that way. Once this happens, more people are likely to try to speak that way. Eventually, people might be punished (e.g., despised) for not having the right accent. Thus, what starts out as a signal about one person's background can become a norm for all.[8]

The signaling principle helps explain how an "is" becomes an "ought." As more and more people use the signal to gain information about others, more and more people will adopt the behavior that leads to being treated well. Gradually the signal will change from indicating a rare person to indicating a common person. On the other hand, the absence of the signal, which originally carried little information, will come to carry substantial information when the signal becomes common. When almost everyone behaves in conformity with a signal, those who don't stand out. These people can now be regarded as violators of a norm—and dealt with accordingly.

Note that there is an important distinction between a convention, which has no direct payoffs one way or the other (such as wearing a tie for men), and a cooperative act, the violation of which leads to injury to others (e.g., queuing for service). A type of behavior with no direct payoffs can become a norm once it develops some signaling value, as is the case when fashion leaders adopt a new style (Veblen, 1899). Once this happens, a violator of this style will be looked down upon. Thus the style will become a norm; individuals will usually follow the style, and those who do not will likely be punished.

The Origin and Content of Norms

Eight mechanisms have now been identified that can serve to support a norm that is already at least partially established. What, however, are the characteristics of the behaviors that arise and then become more and more established as norms? Or to put it another way, just what is the content of behavior that might later turn into a norm?

The answer depends on what types of behavior can appear and spread in a population even when only a few people initially exhibit the behavior. This, in turn, depends on what kind of behavior is likely to be rewarded and punished for its own sake, independently of whether or not it is common behavior.

Two of the supporting mechanisms already considered can serve in this initial role: dominance and reputation. Dominance can work because if only a few very powerful actors want to promote a certain pattern of behavior, their punishments alone can often be sufficient to establish it, even if the others are not vengeful against defections. The implications for the substance of norms are obvious: it is easier to get a norm started if it serves the interests of the powerful few.

In fact, many norms obeyed and even enforced by almost everyone actually serve the powerful. This can happen in forms disguised as equalitarian or in forms that are blatantly hierarchical. An apparently equalitarian norm is that the rich and the poor are equally prohibited from sleeping under bridges at night. A blatantly hierarchical norm is that soldiers shall obey their officers. Both forms are "norms of partiality," to use the term of Ullman-Margalit (1977).

To say that the powerful can start a norm suggests a great deal about the potential substance of such norms. Once started, the strong support the norms because the norms support the strong.

Dominance is not the only mechanism capable of starting a norm. Reputation can do so as well. Consider, for example, the idea of keeping one's promise. In a hypothetical society in which few people kept their promises, you would be happy to deal with someone who did. You would find it in your narrow self-interest to continue dealing with such a person, and this in turn would be rewarding to the promise-keeper. Conversely, you would try to avoid deals with those you knew did not keep their promises. You would, in effect, be vengeful against defectors without having to pay an enforcement cost. Indeed, your enforcement would simply be the result of your acting in your own interests, based upon the reputations of others and your calculation about what was good for yourself.

International regimes depend on just such reputational mechanisms to get norms started (Keohane, 1984). In such cases, countries can be very deliberate about what promises they make and which ones they want to keep when the stakes are high (Axelrod, 1979). Reputational effects can also be based upon the limited rationality of trial and error learning. If a person associates another's response to a particular act (say a refusal to continue dealing as a reaction to the breaking of a promise), then the violator can learn not to break promises.

This learning approach suggests the

importance of being able to link the behavior with the response. Behaviors will be easier to establish as norms if the optimal response of others is prompt and rewarding. Failing a prompt response, learning can also take place if the delayed punishment is explicitly cited as a response to the earlier defection.

Summary and Conclusion

To study the development of norms, the strategic situation has been modeled as an *n*-person game. In the basic norms game, everyone has two types of choice: the choice to cooperate or defect, which affects everyone, and the choice of whether or not to punish a specific person seen defecting. A player's strategy is described in terms of how these choices will be made. A strategy consists of two parameters: boldness (the largest chance of being seen that will lead to a choice of defection) and vengefulness (the probability of punishing someone observed defecting). To the extent that players are vengeful, but not very bold, a norm can be said to have been established.

To study the dynamics of the process, an evolutionary approach was employed. In this approach, the initial strategies are chosen at random, and the population of players is given opportunities to defect and to punish the defections they observe. The evolutionary approach dictates that strategies proving relatively effective are more likely to be employed in the future while less effective strategies are dropped. Moreover, strategies undergo some random mutation so that new ones are always being introduced into the population.

The computer simulation of this process revealed an interesting dynamic in the norms game. At first, boldness levels fell dramatically due to the vengefulness in the population. Then, gradually, the amount of vengefulness also fell because there was no direct incentive to pay the enforcement cost of punishing a defection. Once vengeance became rare, the average level of boldness rose again, and the norm completely collapsed. Moreover, the collapse was a stable outcome.

This result led to a search for mechanisms that could sustain a partially established norm. One possibility is the metanorm: the treatment of nonpunishment as if it were another form of defection; that is, a player will be vengeful against someone who observed a defection but did not punish it. Simulation of the evolution of strategies in this metanorms game demonstrated that players had a strong incentive to increase their vengefulness lest they be punished by others, and this in turn led to a decline of boldness. Thus, metanorms can promote and sustain cooperation in a population.

Other mechanisms for the support of norms are also important. These include dominance, internalization, deterrence, social proof, membership, law, and reputation. In some cases, the resulting norms are hierarchical rather than equalitarian, and the cooperation exhibited is coerced rather than freely offered. A good example is the norm of black deference in the old South.

Dominance processes have been simulated by subdividing the population and letting one segment be relatively resistant to the effects of punishment by members of the other segment. Internalization can be investigated by studying the effects of making defection costly rather than rewarding for some of the defectors and by making punishment a pleasure rather than a cost for some of the observers of a defection. A more drastic change in the modeling procedures would be necessary to study some of the other mechanisms in question.

Norms are important in society and, not surprisingly, have been given a great deal of attention in the social sciences, including sociology, anthropology, political science, psychology, and economics.

While descriptions of actual norms abound, investigations of the reasons for people to obey or violate a given norm have been much less common. Even among the strategic approaches to norms, relatively little attention has been devoted to understanding the dynamics of norms: how they can get started, how a partial norm can be sustained and become well established, and how one norm can displace another. An evolutionary approach is helpful in studying these dynamics because it can help show how strategies change over time as a function of their relative success in an ever-changing environment of other players who are also changing their own strategies with experience.

A major goal of investigating how cooperative norms in societal settings have been established is a better understanding of how to promote cooperative norms in international settings. This is not as utopian as it might seem because international norms against slavery and colonialism are already strong while international norms are partly effective against racial discrimination, chemical warfare, and the proliferation of nuclear weapons. Because norms sometimes become established surprisingly quickly, there may be some useful cooperative norms that could be hurried along with relatively modest interventions.

Notes

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1. The procedure used is inspired by the genetic algorithm of computer scientist John Holland (1975, 1980).

2. For convenience, it is also assumed that the chance of being seen not punishing is the same as the chance of the original defection being seen. The payoff for metapunishment is $P' = -9$, and the metaenforcement cost is $E' = -2$.

3. Marx goes as far as to say that social norms are merely reflections of the interests of the ruling class, and the other classes are socialized into accepting these norms under "false consciousness."

4. I thank David Yoon and Lynn Sanders for pointing this out to me.

5. I thank David Yoon for formulating this variant of the metanorms game and the application to alliances that follows.

6. The same process of formalizing norms applies to private laws and regulations, as in the case of a business that issues an internal rule about who is responsible for making coffee.

7. For the theory of signaling, see Spence (1974). For a theory of how customs can be sustained by reputations, see Akerlof (1980).

8. Signals can also help to differentiate groups and thereby maintain group boundaries and cohesiveness.

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REASON AND WAR

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A new specification of the expected utility model of international conflict places expected utilities within a polar coordinate system; treats third-party choices in a manner more consistent with classical forms; estimates the expected utilities derived from not challenging existing policies; more fully represents the expected costs of conflict; and normalizes expected utilities regardless of system size. By assuming that the probability of escalation of a dispute increases monotonically with leaders' expectations of gain, we derive continuous functions for the probabilities of war, intervention, violence, and peace. The revised theory significantly improves our ability to discriminate between violent and nonviolent disputes and between violent disputes that escalated to warfare and those that did not in Europe between 1816 and 1970.

In this essay, we set out a general theory of the escalation of conflict and test portions of that theory on a broad data set. Because the subject is so large, this paper represents only a small portion of a much more comprehensive undertaking. Additional parts of the theory (and additional tests) are described in Lalman (1985) and in forthcoming papers by Bueno de Mesquita and Lalman.

Assume that a senior foreign policy leader in each country formulates and implements national policy as it relates to decisions to escalate international disputes or to negotiate for a resolution of them. Assume that such key leaders are expected utility maximizers who estimate the welfare that will devolve on their state in the course of such disputes. Assume also that they estimate the utility expected by their opponent in the dispute. Assume that the higher one's estimate of an adversary's expected utility, the greater the costs the adversary is presumed to be prepared to inflict in pursuit of its objectives (Bueno de Mesquita, 1983).

Let i be the initiator of a dispute, and let j be the opponent. In our notation, j's expected utility calculations are represented by superscript j, and i's calculations are denoted by superscript i. Following this rule, $E^i(U_{ij})$ denotes the net utility i expects to gain (or lose) from the dispute with j. The object of the dispute for i is to bring j's policies more into line with those of i. This conflicts with the interest of j, which is to compel i to follow the policy objectives of j. The estimate made by i of the net change in j's expected utility that would be achieved by continuing a dispute with i, denoted $E^i(U_{ji})$, forms the basis for i's anticipation of the behavior of j. Of course, j is assumed to make equivalent calculations. Assume that the larger i's (or j's) expected utility value, the

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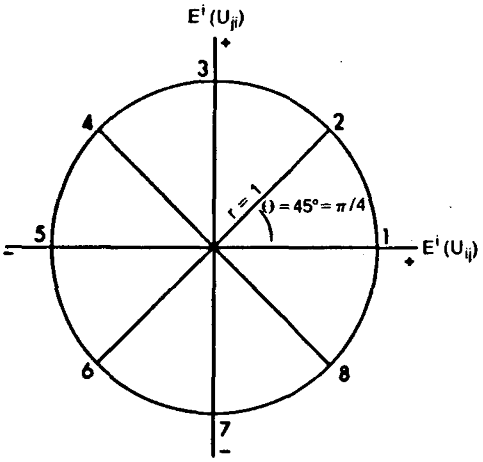
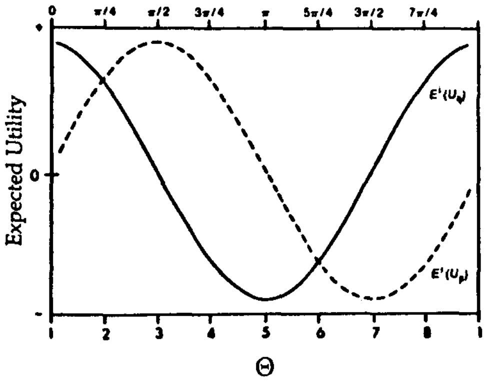
Figure 1. Expected Utilities in a Polar Coordinate System



more inclined *i* (or *j*) will be to use force rather than to negotiate to attain its objectives. In particular, assume that the preference for choosing the strategy of using force rather than the strategy of negotiation increases as a monotonic function of the relevant actor's expected utility.

Figure 1 depicts a system of coordinates in which we can represent *i*'s two expected utility estimates. In previous research, Bueno de Mesquita (1985) developed an ordinal theory of conflict escalation based on the interpretation of this system using rectangular coordinates. Here, we make use of Lalman's (1985) interpretation of this space as a system of polar coordinates to elaborate a continuous theory of conflict escalation. As Figure 1 illustrates, any pair of expected utility values (and the associated point described by the pair) can be generated as an angle and a vector length. Let *r* denote the vector length from the origin to the point, and θ denote the angle from the horizontal axis. All points in the space can be represented by varying *r*, θ, or both. For ease of presentation, we begin our analysis by allowing the angle to vary, while holding *r* constant at 1, so that Figure 1 illustrates the resulting unit circle.

Figure 2. Expected Utilities as Functions of Θ



By unravelling the unit circle from Figure 1, we can more easily depict underlying relationships imbedded in *i*'s (or *j*'s) perception of its own expected utility and that of its potential antagonist. Figure 2 shows the values of these variables for all points along the unit circle. Thus, holding *r* constant and allowing θ to vary produces a cosine curve associated with *i*'s expected utility and a sine wave associated with *j*'s expected utility as assessed by *i*. Allowing *r* to increase has the effect of increasing the amplitude of the cosine and sine functions. Therefore, θ fixes the location of a point on the two curves, and *r* establishes the amplitude of the curves.

When a dispute is initiated, two basic courses of action are open to each of the opponents. Each may choose, based on the respective expected utilities, to escalate the conflict by using force or to deescalate the conflict by offering to negotiate. We assume that *i*'s (or *j*'s) probability of escalating increases as a monotonic function of the expected utility of *i* (or *j*). This assumption stipulates that *i* prefers more utility to less (thus, larger values of $E^i(U_{ij})$ are preferred to smaller values) and that *i* is more willing to use force in pursuit of a larger gain than in pursuit of a smaller one. It is akin to the common assumption in economics that "more is preferred to less." The assumption merely implies that the more i believes it stands to gain, the more willing i is to fight for those gains.

Functions f and g describe, respectively, the inclination of i and j to escalate. Using f and g we transform the functions in Figure 2 to represent escalatory functions (denoted $P^i(Esc_i)$ for i's probability of escalating the dispute and $P^i(Esc_j)$ for j's probability of escalating the dispute, as perceived by i):

$$P^i(Esc_i) = f(r * \cos\theta) \qquad (1)$$

$$P^i(Esc_j) = g(r * \sin\theta). \qquad (2)$$

By the Pythagorean Theorem,

$$r = [E^i(U_{ij})^2 + E^i(U_{ji})^2]^{.5}$$

and by the rules of trigonometry,

$$\cos\theta = \frac{E^i(U_{ij})}{r},$$

so that

$$P^i(Esc_i) = f(r * \cos\theta)$$

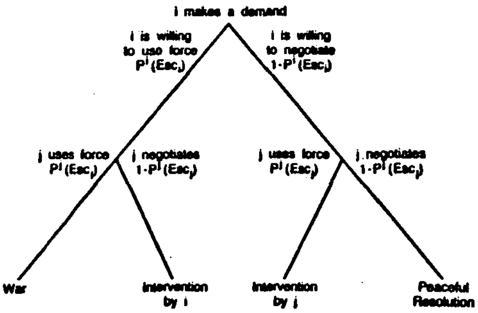
$$= f\{r * [E^i(U_{ij})/r]\} = f[E^i(U_{ij})],$$

and similarly,

$$P^i(Esc_j) = g(r * \sin\theta) = g[E^i(U_{ij})].$$

These expressions demonstrate that the use of polar coordinates has not altered our assumption that the probability of a nation choosing to escalate a dispute is monotonic with its expected utility.

Figure 3. Strategic Responses to Threatening Demands and the Probabilities of Specific Outcomes


i makes a demand
i is willing to use force $P^i(Esc_i)$
i is willing to negotiate $1 \cdot P^i(Esc_i)$
j uses force $P^j(Esc_j)$
j negotiates $1 \cdot P^j(Esc_j)$
j uses force $P^j(Esc_j)$
j negotiates $1 \cdot P^j(Esc_j)$
War
Intervention by i
Intervention by j
Peaceful Resolution

The escalation functions $P^i(Esc_i)$, $P^i(Esc_j)$, $P^j(Esc_i)$, and $P^j(Esc_j)$ may be used to deduce a broad array of generalizations. Let us begin by examining the strategic options open to j once i initiates a demand. The decision tree in Figure 3 shows that i can make a demand accompanied by an offer to negotiate or accompanied by the use of force; j responds to i's demand either by selecting the "force" strategy or the "negotiate" strategy.[1] If, at some point in the process, both i and j choose to use force, then, by definition, war results. If both choose to negotiate, then, by definition, the ongoing dispute is resolved peacefully. If one chooses to escalate while the other opts for negotiations, then, by definition, an asymmetric use of force (which we call an *intervention*) results in which one side uses violent means and the other does not.

We now specify four conditions based on Figure 3:

1. $P(War) = P^i(Esc_i) * P^j(Esc_j)$

2. $P(Intervention)$
$$= \{P^i(Esc_i) * [1 - P^j(Esc_j)]\}$$
$$+ \{P^j(Esc_j) * [1 - P^i(Esc_i)]\}$$

3. $P(Peace)$
$$= [1 - P^i(Esc_i)] * [1 - P^j(Esc_j)]$$

4. $P(Violence) = 1 - P(Peace)$
$$= P(War) + P(Intervention).$$

Clearly, all four probabilities can be calculated directly from knowledge of the relevant expected utility values and knowledge of f and g. At this point in our research, the expected utility values can be estimated according to the formulation described below. The functions f and g, however, remain unknown. Here we construct f and g as equivalent linear transformations in order to estimate values for the probability of choosing the strategies of fighting or negotiating. We assume

$$P^i(Esc_i) = P^i(U^i(I_{ij}) + 3/6,$$

and

$$P^j(Esc_j) = (E^j(U_{ji})+3)/6.$$

These transformations are chosen for their properties as well as their computational convenience. Under our formulation of the calculus of expected utility, values may be obtained in the range +3 to −3. Probabilities, of course, must be in the range 0 to +1. The monotonic functions we have chosen map expected utilities into the [0,1] interval while preserving .5 as the probability of escalation when the expected utility equals 0 (indicating indifference between strategies). These functions also have the characteristic of reaching their limiting values of 0 and +1 when the relevant expected utility attains its minimum or maximum value, respectively. Lalman (1985) examines alternative, logistic forms for *f* and *g* to capture the economic notion of declining marginal utility. Eventually we hope to allow *f* and *g* to vary from decision maker to decision maker. For now we have chosen an approach that falls in the middle of functions that satisfy monotonicity.

Each of the probability terms delineated above can be estimated to reflect the "objective" likelihood of the relevant event, or each can be calculated from the perspective of *i* or of *j* to give that actor's view of the likely evolution of their dispute. The three sets of resulting probability functions would display the differences between the participants' perceptions of their situation and the situation itself. We leave a full discussion of these important perceptual differences to other papers and here merely call the reader's attention to this feature of the model. In the rest of our discussion, we focus on the "objective" probability functions. These are simply the product of *i*'s view of what *i* can do and *j*'s view of what *j* can do.

Although each of the probability functions is derived from only the expected utility estimates of the relevant decision makers and from functions *f* and *g*, each has its own unique shape and properties. From these characteristics it is possible to deduce several important propositions. We leave for other papers rigorous proofs of most of these propositions but, again, mention some here in passing to draw the reader's attention to the broad potential of this approach. We also note what we believe is a very significant—though now seemingly obvious—insight from Figure 3. War and peace have been discussed in the literature as if they were the two ends of a dichotomy in which one or the other must obtain. Frequently, we have heard the statement "peace is the absence of war" as the justification for a dichotomous war/no war variable in analyses. Blainey (1973, pp. 4, 245), for instance, notes that "any document which discusses an international crisis that ended peacefully is a mirror of both peace and war"; he later adds, "War and peace are more than opposites. They have so much in common that neither can be understood without the other." Yet, the theoretical framework set out here (and in Lalman, 1985) shows clearly that $P(War) \neq 1 - P(Peace)$ and that the logic driving intermediate forms of disputes is readily specifiable and calculable from an expected utility framework. Intermediate outcomes that are neither war nor peace are important in specifying tests of our theory. They are also important because while such outcomes have been observed empirically by others (e.g., Gochman, 1975), they have remained largely unexplained until now.

Before turning to the empirical analysis of several important results from the theoretical structure specified here, let us examine a small number of important propositions.

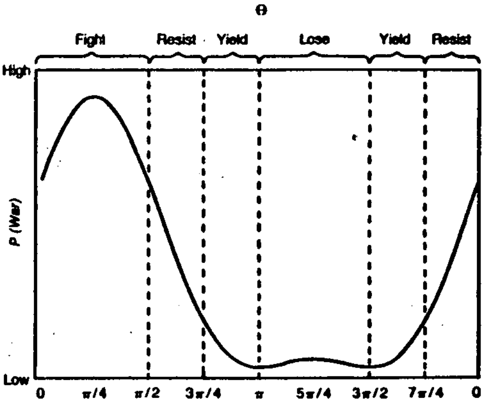
PROPOSITION 1. *If* f *and* g *are strictly monotonic, then necessary and sufficient conditions for war exist only in the limit.*

Proof. In order for necessary and sufficient conditions for war to exist, *P*(*War*) must equal 1. There is a probability of escalation associated with any given expected utility value. Except in the limit, it is always possible for *P*(*War*) to be larger. This value increases as r gets longer if $\theta < 90^\circ$ or if r gets shorter when $135^\circ \leqslant \theta$, and θ is not more than 215°. At other angles, changes in amplitude can increase or decrease the probability of war depending upon the actual size of r and the specific functions of f and g. Since the amplitude of the relevant escalation function can always shift upward, given strict monotonicity, the probability of escalation associated with the previous expected utility value must have been less than 1.0. Now, necessary and sufficient conditions are satisfied if and only if both parties' probabilities of escalation simultaneously equal 1.0, but we have just seen that with monotonicity neither equals 1.0, except in the limit.

PROPOSITION 1a. *For any* f *and* g, *except in the limit, the necessary and sufficient conditions for peace do not exist.*

Proof. Since $P^i(Esc_i) = 1$ only in the limit, it follows that $1 - P^i(Esc_i)$ equals 0 only in the limit. The same, of course, is true for $P^j(Esc_j)$. Now, for necessary and sufficient conditions for peace to obtain, *i*'s and *j*'s probability of escalating must equal 0. This condition only arises in the limit. Therefore, except in the limit, *P*(*Peace*) $\neq$ 1, and so the necessary and sufficient conditions for peace do not obtain. Peace is never inevitable.

Figure 4. Probability of War as a Function of Θ

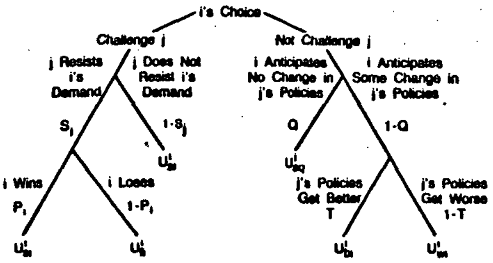


PROPOSITION 2. *The ordinal pattern of the probability of war reported earlier by Bueno de Mesquita (1985) is a special case of the continuous theory set out here.*

Example. Figure 4 displays the probability of war function in generic form, with lines cutting across the function at the boundary between each of the escalatory conditions set out in Bueno de Mesquita (1985). It is evident that the area Bueno de Mesquita called *fight* has a higher probability of war than the area he called *resist*, which, in turn, has a higher probability of war than the area he called *yield*. Finally, this area is higher in terms of the probability of war than is the region he called *lose*.

From the probability functions derived here, it is possible to show when a balance of power (or preponderance) increases the likelihood of peace and when it increases the likelihood of war. With this theory, then, the controversy over the competing hypotheses that peace follows either from a balance of power or from preponderance can be resolved. Likewise, the model allows us to deduce the properties of a power transition that yields war and one that yields peace. Also, the theory allows one to show when an arms race decreases or increases the chance for peace and when arms control increases or decreases

Figure 5. The Decision Problem: To Challenge or Not to Challenge



the chance for war. The simple theoretical structure set out here and elaborated more fully in Lalman (1985) encompasses structural theories of power distributions, arms races, deterrence, negotiations, bluff, and escalation. It is an important elaboration of earlier work in that it is now possible, at least in theory, to explain variance within as well as between quadrants in the coordinate system described here and in Bueno de Mesquita (1985). It is also possible to incorporate many theories of international conflict, whether focused at the level of individual decision makers or at the level of systemic attributes, into this single framework. However, the efficacy of the theory remains to be demonstrated. That is the task to which we now turn our attention.

The Measurement of Expected Utility

Prior to the onset of a dispute, *i*'s decision problem is to choose between challenging *j*, with the intention of convincing *j* to alter its policies to be more in line with those desired by *i*, and not challenging *j*, leaving *j*'s policies to unfold in whatever manner they are expected to unfold in the absence of pressure from *i*. Figure 5 depicts *i*'s decision problem.

If *i* chooses not to challenge *j*, then one of three contingencies may arise. First, *i* may anticipate that with some probability (Q^i), *j* will not alter its current policies over the time period of concern to *i*, and so *i* will derive whatever utility it receives from the preservation of the status quo between itself and *j* (U^i_{sq}). Alternatively, *i* may anticipate that *j*'s policies will change, in which case there is some probability (T) that, from *i*'s perspective, the policies of *j* are anticipated to improve (with U^i_{bi} being the associated utility) or to get worse (U^i_{wi}). When *i* contemplates challenging *j*, on the other hand, *i* must take into consideration the probability that *j* will not simply give in to *i*'s policy demands (S_j), the likelihood that *i* will succeed in its efforts to enforce its demands (P_i), and the utility associated with success (U^i_{si}) and with failure (U^i_{fi}) in those efforts. The overall calculus of expected utility, then, is

$$E^i(U_{ij}) = E^i(U_{ij})_c - E^i(U_{ij})_{nc}, \tag{3}$$

where the expected utility from challenging *j* equals

$$E^i(U_{ij})_c = S_j[P^i_i(U^i_{si}) + (1 - P^i_i)(U^i_{fi})] + (1 - S_j)(U^i_{si}), \tag{4}$$

and the expected utility from not challenging *j* equals

$$E^i(U_{ij})_{nc} = Q(U^i_{sq}) + (1 - Q)[T(U^i_{bi}) + (1 - T)(U^i_{wi})]. \tag{5}$$

Of course, *j* calculates a comparable equation that reflects *j*'s view of the relative merits of challenging or not challenging *i*. Related to these calculations of expected outcomes are calculations of expected costs associated with the pursuit of the desired outcome, which, as noted earlier, we assume is an increasing function of how *i* (or *j*) thinks its adversary will respond to a challenge—that is, $E^i(U_{ji})$ and $E^j(U_{ij})$.

Estimating the "Challenge" Option

The method used for estimating the utility of success (the U_s terms) and of failure (the U_f terms) on the "challenge" side of the calculus is explained in Bueno de Mesquita (1985) and elaborated more fully in Bueno de Mesquita, Newman, and Rabushka (1985). The procedure relies on the use of formal alliance data in the European region. The similarity of alliance portfolios is estimated and used as the basic building block for the utility measures. Nations with more similar alliance portfolios are assumed to have higher utility for each other's foreign policy objectives. Utility diminishes monotonically, though not linearly (because of the inclusion of a risk-taking component), as the alliance portfolios become more dissimilar. The estimates of risk-taking propensities among decision makers are taken from Bueno de Mesquita (1985). The procedure assumes that nations whose leaders have adopted foreign policies that leave them near the extreme of their possible range of vulnerability are more accepting of risk than those who locate their foreign policies in a manner that diminishes their vulnerability to external threats (Bueno de Mesquita, 1985; Iusi and Bueno de Mesquita, 1985; Newman, 1982). Differences in risk-taking propensities are viewed as the source of variations in actor perceptions.

The probability of success terms on the "challenge" side of the equation are derived from Correlates of War composite capabilities data (a file of data we acquired from the Inter-University Consortium for Political and Social Research, University of Michigan). The construction of these terms is predicated on the notion of gambling odds so that the proportion of power on one side or the other is used as the estimate of that side's probability of success. Needless to say, alternative formulations are appropriate and will be tested in the future. Especially germane would be functional forms that have a logistic shape. The terms measuring the likelihood of an adversary's not giving in to a demand (S_i in j's calculus and S_j in i's are assumed to equal 1.0, meaning that i's (j's) demand is highly contentious (and will be resisted).

In one critical respect, the estimate of probabilities on the "challenge" side of the equation departs markedly from earlier attempts. This departure is linked to the perspective that is taken with regard to the contribution third parties make to the expected utility of i (or j). Because of the manner in which third parties are treated in earlier studies (Bueno de Mesquita, 1981, 1985), the operational estimate of expected utility values for any decision maker could vary between $(2N - 2)$ and $-(2N - 2)$, where N is the total number of nations in the relevant international system. Variations in the size of the international community, then, affected the range of possible values in the expected utility models set out earlier. This is a serious shortcoming in that it makes the comparison of a single nation's expected utility scores in different years difficult. A score of .5 in a system of 10 nations simply does not mean the same thing as a score of .5 in a system of 20 nations, and how those numbers relate to one another is not straightforward. The new formulation fixes the range of values, irrespective of system size, in a theoretically meaningful way. To understand how this is accomplished, let us examine the logic behind third-party choices and what potential opponents might estimate about third-party decisions.

Let us assume an ongoing conflict between some nation i and some nation j. Assuming no conflict will end in a true draw, it is then the case that only two outcomes are possible: i will win or j will win. Thus we stipulate two outcomes: W_i and L_i, where the first refers to the outcome in which i wins, and the second refers to the outcome in which i loses (or j wins). Third party k can choose to intervene in the conflict or remain outside the dispute. We assume abstention is the consequence of k's indifference to W_i and L_i.

Figure 6. Third-Party Decision Problem

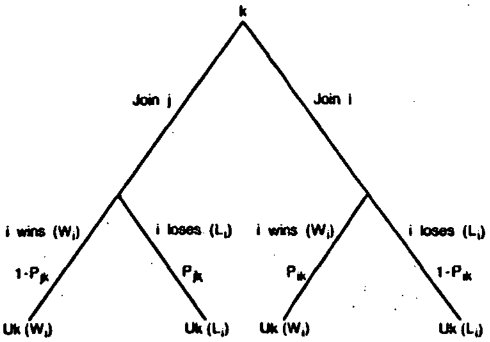


Figure 6 portrays *k*'s decision problem.

According to the model depicted in Figure 6 (see also Altfeld and Bueno de Mesquita, 1979), *k*'s choice between joining *i* and joining *j* depends on the probability of *i* winning, given that *k* helps *i* (P_{ik}); the probability of *i* losing, even though *k* helps *i* ($1 - P_{ik}$); the probability of *j* winning (*i* losing) given that *k* helps *j* (P_{jk}); the probability of *j* losing (*i* winning), even though *k* helps *j* ($1 - P_{jk}$); and the utility *k* attaches to the two possible outcomes. Expressed algebraically, *k*'s expected utility for joining *i* or *j* equals

$$E(U)_k = [P_{ik} U^k(W_i) + (1 - P_{ik}) U^k(L_i)] - [(1 - P_{jk}) U^k(W_i) + P_{jk} U^k(L_i)] \quad (6)$$

If equation (6) is positive, *k* expects more utility from joining *i* than *j*. If it is negative, *k* expects more utility from joining *j* than *i*, and if the expression equals zero, then *k* is indifferent between *i* and *j* and so abstains from the dispute. The terms in equation (6) may be rearranged by factoring to yield

$$E(U)_k = [P_{ik} + P_{jk} - 1] \cdot [U^k(W_i) - U^k(L_i)] \quad (7)$$

Assume no nation joins a conflict with the expectation of harming the side it chooses to join so that the a priori probability of *i* winning if *k* abstains is not larger than the probability of *i* winning if *k* joins *i*, and likewise the a priori probability of *j* winning is not diminished by *k* joining *j*. That is, we stipulate that

$$P_{ik} \geqslant P_{ib};\; P_{jk} \geqslant P_{jb} = (1 - P_{ib}), \quad (8)$$

where P_{ib} and P_{jb} are the respective probabilities of *i* and *j* winning a *strictly bilateral* dispute (as estimated by *k*).

From equation (8) it is evident that $(P_{ik} + P_{jk} - 1)$ is always greater than or equal to zero. After all, $P_{ik} + P_{jk} \geqslant [P_{ib} + (1 - P_{ib})]$, and $[P_{ib} + (1 - P_{ib})] = 1$. From this it follows that equation (7) can be negative only if $U_{ki} < U_{kj}$. Consequently, *k* chooses to join *j* strictly on the basis of *k*'s preference for *j* over *i*. Likewise, *k* joins *i* strictly as a function of *k*'s preference for *i* over *j*. How valuable *k*'s participation is on either side depends, not only on *k*'s utility for one or the other initial belligerent, but also on *k*'s probability of altering the outcome. This is exactly what is expressed by equation (7). Indeed, empirical support for this generalization and for the efficacy of equation (7) is found in Altfeld and Bueno de Mesquita (1979).

Once P_{ib} and P_{jb} (which together equal 1.0 and represent the probabilities when *i* and *j* act alone) are subtracted (as dictated by $[P_{ik} + P_{jk} - 1]$), all that remains is *k*'s marginal contribution to the probability of the outcome. This can be seen most easily by adding an operational assumption. Let $P_{ik} = (C_i + C_k)/(C_i + C_k + C_j)$, where *C* refers to the capabilities or power of the subscripted actor. Similarly, let $P_{jk} = (C_j + C_k)/(C_i + C_j + C_k)$. Then,

$$(P_{ik} + P_{jk} - 1) = \frac{C_i + C_k}{C_i + C_j + C_k} + \frac{C_j + C_k}{C_i + C_j + C_k} - \frac{C_i + C_j + C_k}{C_i + C_j + C_k} = \frac{C_k}{C_i + C_j + C_k}.$$

The above results show that *k*'s preferences determined its choice and that the expected value of that choice is determined jointly by the intensity of *k*'s preference for one side or the other and *k*'s capacity to affect the outcome of the conflict between *i* and *j*. Let us assume that *k*'s effort on behalf of its preferred side is proportional to *k*'s expected utility from participation on that side. Of course, *i* (or *j*) cannot be certain of the intensity of *k*'s preference, although the side *k* prefers can be viewed as known. However, just as *i* can estimate *j*'s expected utility, so too *i* can estimate *k*'s expected utility for joining *i* or joining *j*. Substantively, such an estimate evaluates the reliability of potential allies of *i* and *j* and is critical to *i*'s assessment of its probable success in a dispute with *j*.

We assume that *i* and *j* each are prepared to commit all their resources to their dispute.[2] The probability of success for *i*, then, is a function of *i*'s power combined with the contribution *i* expects from third parties that support *i*, relative to the total power that is expected to be in the conflict. We define P_i operationally as

$$P_i^i = \frac{C_i + \sum_{k:ipj} (C_k)[(U_{ki} - U_{kj})/2]^i}{C_i + C_j + \sum_{k \neq i,j} (C_k) \mid [(U_{ki} - U_{kj})/2]^i \mid} \qquad (9)$$

Substantively, the numerator of equation (9) indicates that *i* is prepared to use all of its resources to achieve *i*'s objectives. The numerator also includes *i*'s estimate of the proportion of third-party resources that will be available to *i*. That proportion is based only on those third parties believed by *i* to prefer *i* to *j*. It is estimated as the sum of the products of the capabilities of each such third party times *i*'s approximation of each third party's intensity of preference for an outcome favoring *i* over *j*. The denominator is *i*'s estimate of the total resources expected to be involved in the dispute. Because these are subjective estimates, *i*'s calculation of P_i and *j*'s calculation of that same term need not be equal. To denote the difference between *i*'s and *j*'s perceptions of these values, we use a superscript. Thus, P_i^i is *i*'s estimate of *i*'s probability of success, and P_i^j is *j*'s estimate of that same probability.

Estimating the "No Challenge" Option

The "no challenge" side of the equation, though discussed in passing in Bueno de Mesquita (1985), is estimated for the first time here. To estimate the utilities associated with anticipated changes in policies when such changes have not been, and cannot have been, observed represents a difficult challenge. We have approached the problem by assuming that preferences among alternative policies are single peaked and that war/peace questions can be represented as if they fall on a unidimensional continuum. With these assumptions—and with the usual assumptions of transitivity, connectivity, and rationality—we are able to take advantage of Black's (1958) median voter theorem. In particular, we assume that in the absence of a policy challenge, international issues are resolved by a tendency for policy to drift toward the "median voter," or Condorcet winner position. Of course, we recognize that voting, in the liberal democratic sense, does not exist in international politics. Rather, we have in mind that the "might makes right" shibboleth is a description of "voting" in contests of power and will. The power or capabilities of nations (denoted C_i for nation *i*) represent the quantity of "votes" they could cast in a policy debate. The utility they attach to alternative policy choices is their intensity of preference, or the will to see one outcome prevail over another. Thus, the difference in utilities acts as a discount on their exertion of effort, or their "casting of votes." If a nation's key leader, for instance, were indifferent to the two alternatives of a debate, then none of that nation's power would be spent in trying to influence the resolution of that debate. If the same nation's leader felt intensely about one of the alternatives, then much of its power would be committed to influencing outcomes. Thus, we define the "vote" of each nation in a policy debate over alternatives x and y as

$$V_i = (C_i)\,(U_x^i - U_y^i).$$

By summing this calculation across all nations and repeating it for all pairwise comparisons of alternatives, we find the Condorcet winner. That outcome, as Black (1958) has shown, is the position of the median "voter." We assume that if issues are resolved purely through the pulls and tugs of international discussion, then each nation in the community of states is pulled toward tacit toleration of this outcome. We infer that i's best guess as to the policy j will adopt if i does not directly challenge j is that if j shifts its stance, j will move to the position of the Condorcet winner. This inference assumes that if j is willing to alter its stance, then j will move to a policy position that minimizes j's future vulnerability to defeat.

How likely is j to move to this position? Four approaches to estimating an answer to this question suggest themselves. One approach is to assume that i's best guess is that j will continue to pursue whatever policies j is pursuing at the moment. This is the assumption made in Bueno de Mesquita (1985). A second assumption is that j will continue to change its policies at the same rate as those policies were changing vis-a-vis i over the recent past. This turns out empirically to be equivalent to the first assumption for the events examined here. A third approach assumes that estimates of the probability of j shifting positions is a function of j's risk propensity (or i's perception of that propensity). The fourth approach is to assume that i is maximally uncertain about j's future behavior—that is, i believes j will continue its current policies with probability .5, and j will move toward the Condorcet winner position with probability .5. We adopt the last perspective because it assumes the least information about j's future behavior. Other approaches will be explored more fully in subsequent analyses.

We stipulate that $Q = .5$. We calculate the utility from the status quo in exactly the manner described in Bueno de Mesquita (1985). The Condorcet winner policy outcome is calculated in the manner described above, and i's utility for a shift by j to that position is the difference between i's utility for that position and i's utility for j's current position. If j is on the opposite side of this alternative from i, then i expects to derive a benefit from j's move (and so U_{bi}) is expected with probability 1.0, given that j is expected to change its position—which occurs with probability .5). If j is on the same side of the "winning" position as i (and between i and the "winning" position), then a move by j to that predicted outcome makes i worse off (U_{wi} is obtained with probability 1.0, given that j changes its current position—which occurs with probability .5). If j is on the same side of the "winning" position as i and is farther away from that position than is i, then the utility i attaches to j's move can reflect an improvement or a deterioration in relations between i and j, depending on whether j ends up farther from i's ideal point after the anticipated move than j was at the outset.

Testing the Theory

We test portions of the theory set out here by focusing on 133 conflicts in Europe from 1816 through 1965. These are the same cases that were examined in Bueno de Mesquita (1985). In subsequent studies we hope to expand our analyses to include a broader range of conflict and nonconflict situations from all parts of the globe.

The 133 cases have been categorized

according to the level of escalation the dispute reached. Thirty-four cases became wars, as defined by Singer and Small (1972). Another 48 events escalated to the level of an intervention in which one side used armed force against the adversary (Gochman, 1975). The remaining 51 conflicts were threats (Gochman, 1975) in which neither side escalated beyond talk to direct military action against the adversary. These disputes were resolved peacefully.

We have claimed that this theory represents an improvement over its predecessors. This version has the potential of accounting for variance within as well as between *fight*, *resist*, *yield*, and *lose* situations, where those situations are defined as follows:

Fight = $E^i(U_{ij}) \geqslant 0$ and $E^j(U_{ji}) \geqslant 0$

Resist = $E^i(U_{ij}) \geqslant 0$ and $E^j(U_{ji}) < 0$ and $|E^j(U_{ji})| < E^i(U_{ij})$, or $E^j(U_{ji}) \geqslant 0$ and $E^i(U_{ij}) < 0$ and $|E^i(U_{ij})| < E^j(U_{ji})$

Yield = $E^i(U_{ij}) \geqslant 0$ and $E^j(U_{ji}) < 0$ and $|E^j(U_{ji})| \geqslant E^i(U_{ij})$, or $E^j(U_{ji}) \geqslant 0$ and $E^i(U_{ij}) < 0$ and $|E^i(U_{ij})| \geqslant E^j(U_{ji})$

Lose = $E^i(U_{ij}) < 0$ and $E^j(U_{ji}) < 0$.

As the decision tree in Figure 3 makes clear, disputants must choose between a strategy of negotiation and a strategy of force. War, as an outcome, is never pitted directly against a peaceful resolution because war can only arise when both parties select the option of using force. Thus, the theory dictates that analyses first pit violent disputes (interventions and wars) against nonviolent disputes (threats) and then pit interventions against war. This means that testing of the model requires splitting the data into two overlapping sets, one that has a dependent variable coded 1 for violent disputes (wars and interventions) and 0 for threats and another coded 1 for wars and 0 for interventions. We call the first dependent variable *violence* and the second *war*.

Because our dependent variables are dichotomous, we test the theory by using logit analysis. The tests are performed by estimating logit models controlling for whether the context of the dispute is a fight, resist, yield, or lose situation.[3] In each logit analysis, we estimate whether an event (threat, military intervention, or war) did or did not happen. Logistic regression is, of course, an appropriate estimation technique for models with dichotomous dependent variables.

Proposition 2 shows that the original ordinal model of conflict behavior is a subset of the continuous model set out here. One test of the efficacy of the theory, then, is to determine whether the continuous version is capable of explaining variance in conflict outcomes *within* the ordinal categories. The earlier theory, of course, could only explain variance *between* categories. The test of proposition 2, then, examines whether the new theory does as well or better than the simpler ordinal theory.

Two sets of analyses follow from the examination comparing threats and violent disputes. The first approach ignores our *post hoc* historical knowledge of which cases became violent. This test relies only on the model to separate candidates for violence from all 133 cases and then to further separate those candidates into interventions and wars. The second approach is to use all the information in the data set by focusing in the second instance only on the cases that actually became violent. This approach assumes that decision makers can readily observe that one or another party to a dispute is prepared to use force. The question then is whether that use of force will prompt negotiations or a reciprocal use of force.

In analyses concerned with the choice between war and intervention, the relevant independent variables are *P*(*War*), *P*(*Intervention*), $E^i(U_{ji})$ (as an indicator of costs expected by *i*), and $E^j(U_{ij})$ (as an indicator of costs expected by *j*). The variable *P*(*War*) should, of course, be positively associated with the dependent variable, as the higher this value is, the more likely it is that an ensuing dispute will become a war. Likewise, *P*(*Intervention*), which assesses the probability that one or the other decision maker will choose to use force when his adversary chooses to negotiate, should be negatively associated with the tendency of the ensuing conflict to become a war. The two expected utility values each represent one actor's view of the other actor's expectations. These are, as noted earlier, assumed to be indicators of expected costs. Consequently, each should be negatively associated with the tendency for the relevant actor to escalate the conflict. Each of the probability variables, according to the theory set out earlier, has a different impact under the circumstances described in different parts of the polar coordinate system (reflecting, as they do, different negotiation contexts). This means that we should allow the slope to vary, which we do by creating dummy variables for each of the main areas of the coordinate system (fight, resist, yield, and lose). The theoretical variables —*P*(*War*) and *P*(*Intervention*)—and the two cost variables are multiplied by these dummy variables.

Analysis concerned with the distinction between nonviolent conflicts and violent disputes, according to the theory presented here, requires the specification of a variable we call *P*(*Violent*), which measures the probability of a dispute becoming violent. This variable is defined simply as [*P*(*War*) + *P*(*Intervention*)], these two probabilities defining the only circumstances under which a conflict can escalate. The theory also requires the specification of expected costs, and so we include $E^i(U_{ji})$ and $E^j(U_{ij})$. For this investigation, dummy variable interactions are again required to reflect the contextual change across the four situations specified earlier (Bueno de Mesquita, 1985).

The probability variables, the cost variables, and even the sectoral dummy variables are all derived from four factors: $E^i(U_{ij})$, $E^i(U_{ji})$, $E^j(U_{ij})$, and $E^j(U_{ji})$. The independent variables are derived theoretically, and their presence in the analysis is dictated by theory. Thus, they must all be included to avoid biased categorization of the dependent variable, but there is, of course, an enormous amount of collinearity among them. This makes impossible the reliable estimation of parameters or significance levels for individual variables.[4] Our main concern is with the theory's ability to discriminate between types of conflicts, and that can be reliably estimated using logit analysis. However, because we are sure some readers will want information on the performance of individual variables, we include here correlation coefficients between the probability terms and the dependent variables, and one limited logit analysis that includes probability and cost terms. We are particularly interested in demonstrating the power of the functional forms governing the definition of the probability variables. Thus, in examining the correlation coefficients, particular attention should be paid to the success of the appropriate probability variables in accounting for the relevant dependent variables and the inability of inappropriate probability variables to explain events outside their theoretical domain. Let us turn now to these analyses.

First we report, in Table 1, the simple bivariate correlation between each of the probability variables and whether the dispute became violent. The correlations are interesting in several respects. Note that *P*(*Intervention*) is positively correlated with the dependent variable called *violent* but negatively correlated with the variables that assess whether a dispute escalated to warfare. This is exactly what the theory leads us to expect. Second, observe that the correlation of *P*(*War*) rises stead-

Table 1. Bivariate Correlations between Theoretically Derived Probabilities and the Escalation of Disputes

Dependent Variables	*P*(*Violent*)	*P*(*War*)	*P*(*Intervention*)	*P*(*Peace*)
Violent (N=133)	.31	.23	.16	-.31
War[a] (N=133)	.31	.41	-.27	-.31
War (N=82)	.32	.45	-.52	-.32

[a]The dependent variable *War* is correlated with the independent variables first for all 133 events, and then only for the 82 events known to have become violent (i.e., 34 wars and 48 interventions). The dependent variable *Violent* includes 51 threats, 48 interventions, and 34 wars.

ily as the dependent variable moves closer to the domain of events it should theoretically explain. Thus, *P*(*War*) is most powerful when war is a designated outcome on the dependent variable and does least well when undifferentiated violence is a designated outcome. The same is true for *P*(*Intervention*) when the dependent variable differentiates these two types of conflict. Third, we should observe that *P*(*Violent*)—which is [1 − *P*(*Peace*)]—is the strongest predictor when the dependent variable discriminates only between violent and nonviolent disputes; when the dependent variable is more discriminating, the more specific probability terms emerge as the stronger predictors.

We present one illustrative logit analysis in which we do not control for situational context. Four independent variables are used to discriminate between conflicts that became wars and those that remained at the level of an intervention. The independent variables are *P*(*War*), *P*(*Intervention*), $E^i(U_{ji})$, and $E^j(U_{ij})$.[5] The theory indicates that the first of these variables should be positively associated with escalation to warfare and that the latter three should be negatively associated. This means that when the probability of an intervention is high, the conflict remains an intervention, and when costs are expected to be high, this acts as a force to hinder escalation. The actual logit results are[6]

$$\begin{aligned} \text{War} = {} & -21.27 + \underset{(p\,<\,.02)}{139.78\,[P(War)]} \\ & - \underset{(p\,<\,.40)}{26.35\,[P(Intervention)]} - \underset{(p\,<\,.02)}{10.11\,[E^i(U_{ji})]} \\ & - \underset{(p\,<\,.02)}{10.94\,[E^j(U_{ij})]}. \end{aligned}$$

The overall logit model fits very well, with $X^2 = 32.71$ and the probability of the results having arisen by chance being less than one in a thousand. The Kendall rank order correlation between predicted and actual outcomes is .65 ($N = 82$), and the signs of the coefficients are all in the predicted direction, with only *P*(*Intervention*) being insignificant.

Let us turn now to the results of the fully specified logit analyses. First we examine the predictive capabilities of the theory against the 133 disputes, of which 51 were resolved peacefully and 82 became violent. Table 2 compares the predicted and actual categorization of events. As can be seen, the theory does excellently at isolating the violent disputes but at the expense of over-predicting such events. Of the 82 violent disputes, all but 5 were predicted to be violent by the

Table 2. Predicted Violent and Nonviolent Disputes in Europe, 1816–1965

	Predicted Event	
Actual Event	Nonviolent	Violent
Nonviolent	20	31
Violent	5	77

theory. All 34 wars were correctly categorized as disputes that involved force. The five cases that were erroneously classified as nonviolent were interventions, a lower level of violence. Of 51 threats, 20 were correctly categorized. The quantity X^2 for the model, with 12 degrees of freedom ($N = 133$), is 32, which is significant at about the .001 level.[7] The Kendall rank correlation between predicted probabilities and actual categorization is .42.

With these results in hand, we turn to our two remaining analyses. In one, we use the predictions of the previous analysis to identify the candidates for violence. Those 108 candidates (77 cases correctly predicted to be violent plus 31 incorrectly predicted) are pitted against the theoretical model that should discriminate between disputes that became wars and those that did not. Here we use only the model to identify candidates for violence and then use the model again to discriminate between those conflicts that were likely to become wars and those that were not. Thus, we rely on theoretical predictions to generate subsequent predictions, running the risk of categorizing as wars cases known to be nonviolent. The test is extremely stringent because we carry our errors from the first stage of analysis into the second stage. Despite the stringency, the theory performs very well. Table 3 shows that 63 of the 74 contingent events that did not become wars were predicted not to become wars and that 18 of the 34 wars were correctly categorized. The quantity X^2 for the logit analysis is 39.36, which, with 12 degrees of freedom, is significant at less than the .001 level.[8] The Kendall rank correlation for the predicted and actual outcomes is .70 ($N = 108$).

We turn now to the test that takes advantage of the available information. Here we examine only those cases in which at least one party has revealed its willingness to use force and ask, which of these violent disputes will become wars? With the added knowledge that all of these disputes are violent (whereas the contingent analysis included 31 threats that were mistakenly categorized as candidates for violence), the model should do still better, and it does. Table 4 reveals that 44 of the 48 interventions are correctly idetified and that 27 of the 34 wars are likewise correctly categorized.

Table 3. Contingent Analysis Predicting War and Intervention in Europe, 1816–1965

	Predicted Event	
Actual Event	Intervention	War
Intervention (Or carried-over threat)	63	11
War	16	18

Table 4. Analysis with Information Predicting War and Intervention in Europe, 1816–1965

Actual Event	Predicted Event Intervention	War
Intervention	44	4
War	7	27

This is over 90% of the interventions and nearly 80% of the wars. The result is so strong that the theory contributes a proportionate reduction in error of more than 67%. This means that the theory makes two-thirds fewer errors than would have been experienced if we had predicted that each event would fall in the modal event category of military intervention. The quantity X^2 for the analysis is 72.25, which, with 12 degrees of freedom, is significant at less than the .001 level. The rank correlation is an astonishing .94 ($N = 82$).

The theory presented here has proven to be a powerful tool for distinguishing between violent and nonviolent disputes and, within the category of violent conflicts, between interventions and wars. One final analytic question remains to be addressed: is this a better theory than the ordinal model proposed earlier (Bueno de Mesquita, 1985)? To answer this question we examine the success of the ordinal theory on the same data sets and compare the results. The new theory is superior if it performs significantly better than the earlier version. The strictly ordinal model produces an X^2 statistic of 16.29 when the dependent variable distinguishes only between violent and nonviolent disputes. With 3 degrees of freedom, this is significant at the .001 level. The difference in X^2 for the tests of the ordinal and continuous theories is itself significant at about the .05 level, indicating that the continuous theory set out here is a substantial improvement. This is seen even more clearly when we turn to the performance of the two models on the 82 cases of interventions and wars. The ordinal model again is significantly associated with the actual outcomes at the .001 level, but the difference in X^2 across the two tests is also significant at less than the .001 level. Thus, the ordinal theory correctly predicted 58 of 82 events, representing a 29% reduction in error. As noted, the improved theory correctly categorized 71 of the 82 events, accounting for more than half of the cases that remained unexplained by the ordinal theory, which was itself a highly significant predictor.

Conclusion

We have proposed a continuous theory linking expected utility estimates to conflict escalation. We have shown that the theory is a powerful tool for discriminating among events likely to be violent or nonviolent and especially among events likely to involve only modest levels of force and those that reach the level of warfare. We have shown that important new propositions can be deduced from the theory and have indicated that many other propositions will be derived later. We have demonstrated that peace is not the opposite of war but that an important contingency arises when one adversary chooses to use force and the other chooses to pursue a strategy of negotiation. We have also shown that the expected utility framework is readily able to distinguish the circumstances under which these alternative strategies are chosen. We have demonstrated improved predictive ability resulting from the development of a continuous theory of escalation (see also Lalman, 1985) and an improved specification of the expected utility structure. The revised theory incorporates information more effectively, explicates rational calculations in a manner consistent with the expected utility axioms while making weaker assumptions than earlier efforts; it explains many apparent anomalies reported in the literature on international conflict. We believe we have demonstrated strong, cumulative results that encourage further confidence in an expected utility paradigm of international conflict behavior and rational decision making.

Notes

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1. These strategies are similar, but not identical, to the strategies of "stand firm" and "stand down" investigated by Petersen (1983) in his study of the relationship in certain types of disputes, between expected utilities and escalation to war.

2. Recall that we assume S_j and S_i equal 1.0.

3. The last two (*yield* and *lose*) are combined in the logit analyses that evaluate the likelihood of a dispute to remain an intervention or become a war because there is too little variance on the dependent variable in the *lose* category to make computations possible for that condition by itself.

4. Significance tests are used here as a heuristic device to suggest the relative strength of associations.

5. Recall that [*P*(*War*) + *P*(*Intervention*)] does not equal 1.0. It is [*P*(*War*) + *P*(*Intervention*) + *P*(*Peace*) that equals 1.0—that is, war is not equivalent to the absence of peace.

6. The probabilities reported in parentheses are derived from X^2 values for the individual coefficients estimated in the logit analysis. Multicolinearity is a problem even in this limited logit analysis. Rerunning the analysis dropping either *P*(*War*) or *P*(*Intervention*) leaves the remaining probability variable statistically significant.

7. The independent variables for this analysis were $\mathit{Fight}^*P(\mathit{Violent})$, $\mathit{Resist}^*P(\mathit{Violent})$, $\mathit{Yield}^*P(\mathit{Violent})$, $\mathit{Lose}^*P(\mathit{Violent})$, $\mathit{Fight}^*E^i(U_{ji})$, $\mathit{Fight}^*E^j(U_{ij})$, $\mathit{Resist}^*E^i(U_{ji})$, $\mathit{Resist}^*E^j(U_{ij})$, $\mathit{Yield}^*E^i(U_{ji})$, $\mathit{Yield}^*E^j(U_{ij})$, $\mathit{Lose}^*E^i(U_{ji})$, and $\mathit{Lose}^*E^j(U_{ij})$.

8. The independent variables are $\mathit{Fight}^*P(\mathit{War})$, $\mathit{Resist}^*P(\mathit{War})$, $(\mathit{Yield} + \mathit{Lose})^*P(\mathit{War})$, $\mathit{Fight}^*P(\mathit{Intervention})$, $\mathit{Resist}^*P(\mathit{Intervention})$, $(\mathit{Yield} + \mathit{Lose})^*P(\mathit{Intervention})$, $\mathit{Fight}^*E^i(U_{ji})$, $\mathit{Resist}^*E^i(U_{ji})$, $(\mathit{Yield} + \mathit{Lose})^*E^i(U_{ji})$, $\mathit{Fight}^*E^j(U_{ij})$, $\mathit{Resist}^*E^j(U_{ij})$, and $(\mathit{Yield} + \mathit{Lose})^*E^j(U_{ij})$.

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A SPATIAL MODEL OF INTERNATIONAL CONFLICT

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The model presented here assumes that nations initiate conflict to change the international status quo across one or more issues to an outcome they prefer. Their preferences for different issue outcomes are represented by a classical spatial utility function. International coalitions are formed both to enhance the chance of a successful challenge and to counter existing challenges. Disputes are modeled as periods of competition between two coalitions through the recruitment of additional members, erosion of support for the opposing coalition and escalation of the crisis. The two-nation model is analyzed in detail, and the existence of the core for the resulting game is proved. The two main conclusions of the two-nation model are that nations have a general incentive to exaggerate their issue positions in disputes and that the possibility of issue trades enhances the likelihood of compromise.

Because scientific understanding arises from theories, the explication of theories is essential for the development of scientific understanding. To understand international conflict—that is, the resolution of disputes between nations by coercion or the threat of coercion—a theory that can explain such conflict is needed. This paper will present a model drawn from rational choice theory—specifically game theory—that explains national actions in conflicts as strategic choices made in order to gain preferred resolutions of the international issues in dispute. Compared to existing game theory models of international conflict, the model presented here provides an explicit description of how each nation's evaluation of the issues in dispute affects its desire to pursue compromise or conflict; a more detailed sequence of escalation allowing for the detailed study of escalation strategies; and a framework that can explain the timing and extent of third-party interventions into ongoing crises.

Why a Spatial Model?

The application of game theory to international conflict has not yet supplied a general explanation of crisis behavior, despite some suggestive insights into the nature of crises (e.g., Snyder and Diesing, 1977). There are a number of reasons why this hope has not been fulfilled. First, all game models must assign utilities to the possible outcomes; normally, a utility evaluation is simply given for each outcome. However, rational choice models are most compelling when the actors' utilities for outcomes can be tied to primitive quantities (Simon, 1985), like income or goods in economics or to the resolution of issues, as in the spatial theory of elections. Because the outcomes of international issues do change as a conse-

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quence of international conflict (e.g., territory changes hands and political policies change) and because the entire character of a dispute changes as the issues in dispute change (e.g., a U.S.-Soviet crisis over Nicaragua is fundamentally different from a U.S.-Soviet crisis over the Middle East), each nation's utility for the possible outcomes of a conflict will be identified with the possible issue resolutions of the conflict and the material costs that will be incurred during the conflict. The formal representation of national preferences for possible issue outcomes will be given by spatial utility functions (Davis, Hinich, and Ordeshook, 1970) as commonly used in the spatial theory of elections.

Many additional advantages are gained by adopting spatial utility functions identified with international issues. A much greater diversity of possible outcomes becomes immediately possible under this framework, unlike the four outcomes presented in a 2 × 2 game. The selection of which issues are included in a conflict now becomes an endogenous question in the model: the issues in dispute will be those that the actors include in the dispute for strategic reasons. A detailed picture of the conflict of interest between the parties can be captured by breaking down their differences into separate issues and comparing their positions on those issues with those of third parties. Differences in the actors' evaluations of the importance of the issues and in their willingness to take risks are reflected by a spatial utility function. Because of these advantages, a formalism drawn from the spatial theory of elections is exceptionally appropriate for the representation of national issue preferences here.

The second reason that applying game theory to international conflict has not yet led to a general explanation of crisis behavior is that any game model must constrain and structure the choices it provides the actors. However, many rational choice models, such as 2 × 2 games and Bueno de Mesquita's (1981, 1985) expected utility theory, reduce the diversity of choices to a dichotomy between war and peace, firmness and conciliation. As Wagner (1983, 1984) points out, this reduction of choice to two alternatives may oversimplify the decisions, leading to incorrect predictions. In this model, an escalation ladder replaces the simple choices of war and peace that characterize most rational choice models. The parties are then free to stop at intermediate levels of escalation if they believe that further escalation would be counterproductive. This additional detail also allows this model, unlike other rational models, to capture the dynamics of escalation, creating the possibility of scientific crisis management.

Third, almost every dispute involves more than two actors, if only for the unrealized intervention of a third party. Models based on two-person game theory can only attempt to capture the effects of third parties on conflict by modifying the payoffs or probabilities, as in Bueno de Mesquita's (1981, 1985) expected utility model. A better approach is to include all possible intervening nations as actors in the model, creating an *n*-person game. One focus of the model presented here is the recruitment of uncommitted third parties to the contending sides. Third-party interventions then become endogenous to the model, allowing for the explanation of the timing and magnitude of interventions. Furthermore, because of the model's detailed consideration of the issues in the dispute and its escalation ladder, it can explain why the two sides might shape their issue demands and escalatory actions to encourage or discourage possible third parties from intervening.

There are two previous efforts to use spatial modeling as a means of developing a more detailed rational choice theory of conflict: Morrow (1985a) and Morgan

(1984). The Morrow paper can be considered as a special case of the model presented here, the case of one issue and a one-step escalation ladder. The Morgan paper attempts something rather different: Morgan creates a probability distribution for each side's willingness to accept possible compromises and then derives the probability of each possible compromise and of war. Like Morrow (1985a), Morgan only considers a one-issue case, losing much of the detailed characterization of the issues that a spatial model can provide. Furthermore, Morgan's probability distribution for acceptable compromises is simply assumed (within certain reasonable restrictions), instead of being derived from fundamental terms like the capabilities and issue preferences of the two sides. The model presented here allows the deduction of each side's bargaining position from fundamental terms and could provide a means to derive the probability distributions that power Morgan's model.

This paper will unify three distinct traditions in formal theory—*n*-person game theory, the expected utility theory of war, and spatial modeling—to produce a formal theory of international conflict. The theory developed here produces far more detailed predictions than any of the other approaches alone.

Briefly, this theory assumes that nations act in disputes to obtain policy outcomes preferable to those that would have occurred had they not acted. A coalition (sometimes only one actor) arises to challenge the status quo across one or more issues, in the expectation of improving the status quo. In response to this challenge, a countercoalition (perhaps just one actor) may or may not emerge in the hope of thwarting the challenging coalition. In the sequence of negotiation and threats between the coalitions, escalation and concessions are the tools that each coalition uses to intimidate and entice the members of the other coalition. Eventually, the dispute will reach a resolution when both coalitions either compromise or resort to the application of force. *N*-person game theory describes the internal struggle within a coalition for domination as well as the contest between coalitions for dominance over the contended issues. The members of each coalition will evaluate their positions within their coalitions by considering their expected utilities calculated over the issues as measured by their individual spatial utility functions.

Basic Assumptions

Because this model explains behavior in disputes as a matter of rational choices within a constrained framework, we assume that actors are capable of coherent choice. The employment of any decision theory based on utility theory requires the assumption that each decider has complete, transitive preferences (Luce and Raiffa, 1957; Riker and Ordeshook, 1973; Savage, 1972). For individuals, this assumption is generally not problematic. However, we cannot necessarily expect aggregations of rational individuals to reach consistent decisions (Arrow, 1963). If we assume the preferences of the members of the deciding group are restricted in certain ways, then the group can arrive at a consistent set of preferences over the alternatives (Sen, 1970).[1]

Generally, value restrictedness within the political leadership of a nation is more likely when decisions are reached by a small group. Because crisis decisions are made by the top political leadership of a country, value restrictedness and, therefore, coherent group preferences are likely to be the rule rather than the exception.[2] Consequently, the actors considered by this model will be restricted to modern nation-states. Nations have governments that are responsible for their foreign policy decisions, and those foreign policy actions provide a sufficient base of international policy acts to permit the inference of a set of preferences from those actions.

Having defined the participants, we need to clarify the stakes in international conflict. This model postulates that international conflicts arise over international issues.[3] An *international issue* is defined as a policy or a set of policies of a government that has international implications and for which all the actors' preferences concerning the outcome are single-peaked; that is, each actor has a single preferred outcome (called its *ideal point*), and an actor's preference for outcomes of an issue drops off as the distance between the ideal point and the outcome increases. Issues provoking conflict include territorial disputes (the policy in question being the possession of a piece of land), external political policies, and those internal political policies that are viewed by at least one government as having international ramifications. For instance, the conflict between Iraq and Iran that has smoldered through their five-year war covers the territorial issue of possession of the Shatt al-Arab waterway, the Iranian policy of Islamic fundamentalist agitation in the Arab world, and the composition of the Iraqi government.

The definition of issues in this model is left deliberately vague to allow the actors to define what issues are relevant to international politics. Each individual issue is cast as a continuum representing the different positions that could be adopted on that issue. The entire realm of issues that could provoke conflict can then be represented formally by an m-dimensional Euclidean space, as is common in spatial voting theory. For convenience, the status quo over all the issues will be defined as the origin.

Each nation's ideal point gives the complete set of issue positions preferred by that nation over all other complete sets of positions. For any nation i, its ideal point will be denoted x_i. Each nation's preferences for issue outcomes will be represented by a utility function based on the weighted distance of each point from its ideal point. The weights attached to the different issues reflect the relative saliency of each issue from the perspective of that nation. These weights also allow for the interactive effects of multiple issues, representing the perceived dependence of the outcomes of some issues on the outcomes of others.

Each nation's utility function also incorporates its willingness to take risks, as exhibited in the curvature of its utility function. Those nations unwilling to take risks, referred to as *risk-averse*, have concave utility functions, while those nations willing to take risks, called *risk-acceptant*, have convex utility functions. According to this approach, the willingness of a nation to take risks arises out of the difference between its expected utility for a gamble and its utility for a sure thing (Friedman and Savage, 1948).

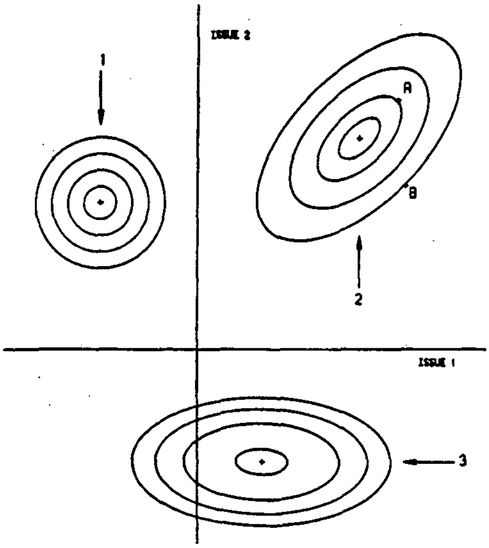
The general form of the utility function will reflect differences in (1) ideal points, (2) in the salience of issues, (3) in combinations of issues, and (4) in attitudes toward risk. Formally, the following utility function allows for all four of these variations within its structure:

$$u_i(x) = -[(x - x_i)\,A_i\,(x - x_i)^T]^{\frac{1}{2}r_i}, \quad (1)$$

where x_i is nation i's ideal point, A_i is an m-by-m positive matrix, and r_i is nation i's risk attitude.[4] An actor's utility for an outcome is the weighted distance between that outcome and the ideal point altered by the risk attitude of the actor.

Sample utility functions in a two-dimensional issue space are given in Figure 1. Each function is represented by a set of indifference curves. All three utility functions have different ideal points given by the cross in the center of each set of curves. The A_i matrix produces both the eccentricity and the orientation of the major axis of the elliptical indifference curves seen in the second and third cases. In the first set of indifference curves, the A_i matrix is equal to the identity matrix, producing circular indifference curves and reflecting preferences that view the issues as separable (the outcome of one issue is irrelevant to the desired outcome of the other) and equally salient. The third set of curves reflects the view that the issues are separable but that the issue given on the vertical axis is more salient than the issue given on the horizontal axis (a given change in the vertical direction produces a greater change in utility than the same change in the horizontal direction). The second set of indifference curves represents an evaluation that the issues are equally salient but not separable; the outcome of one issue determines what outcomes are desirable for the other. In this case, outcomes in which the resolution of both issues lies above or below the actor's ideal outcome for that issue, like point *A*, are preferred to outcomes in which one resolution is above and the other below, like point *B* (i.e., the issues are complementary).

Figure 1. Three Possible Spatial Utility Functions



The risk attitudes of different actors are captured by the curvature of the utility surfaces, which will be reflected in the spacing of indifference curves with equal drops in utility between each pair of curves. The first set of indifference curves depicts risk-neutral preferences because the curves are equally spaced; equal losses in issue space create equal losses in utility. The second set of curves gives the pattern of a risk-acceptant actor; its utility drops off quickly around its ideal point and then levels off. Consequently, an action providing a chance at its ideal point at the cost of a chance at an outcome far from the ideal point will be preferred to the certainty of a moderate outcome. Risk-averse preferences are mirrored in the third set of indifference curves. Outcomes broadly spaced about the third ideal point are valued relatively equally; then the valuation of outcomes rapidly drops off. As a result of this pattern, moderate compromises will be preferred to gambles. Although the structure of the utility function is complex, its components are simple and yet general.

Now that the stakes of conflict and the reason for conflict have been specified, the means of conflict are needed to complete the basic assumptions of the model. Each coalition will have the choice of many different means of pressing an opposing coalition in order to gain its desired outcome. The available means to coerce an opposing coalition include various applications of military force, economic sanctions, and political penalties.[5] For each of these different means of coercion, each nation is considered to have a capability score that measures its ability to use each specific means to gain outcomes in the policy space. These means of coercion are ranked in an "escalation ladder" (Kahn, 1965), with the level of coercion increasing with each step up the ladder. The means of coercion are ranked, then, from first to last according to the strength of their coercive power, as measured by their ability to seize policy outcomes in the absence of the agreement of the opposition.

Formally stated the preceding assumptions are as follows:

> There exists a set of n nations; N; a compact and convex issue space, Q, with $Q \subseteq R^m$; and a status quo point, $x_{SQ} \in Q$, which is set equal to 0. Each nation, i, has a utility function, u_i, as defined in equation (1), that specifies its preferences for all outcomes in the issue space Q. There also exists a finite, well-ordered set of p coercive means, π.

Note that the issue space is defined as bounded; this prevents, among other possibilities, infinite utilities arising from the utility functions. For convenience, the status quo issue disposition can always be moved to 0 by translation of the axes. The assumptions about the utility functions and the means of coercion have been discussed above.

These assumptions limit and define the basis of the model: what actors we are analyzing, what they are competing for, how they value the outcomes, and what means they have available to gain those outcomes. Now the model's coalitional structure needs to be detailed.

The Contest of Coalitions

Coalitions are central to any n-person game. In simplest terms, a *coalition* is a collection of actors who agree to coordinate their strategies for their mutual benefit.[6] In terms of the model presented here, a coalition will have to coordinate both the means of coercion employed by the actors and their proposed resolution of the issues. Coordination of the means of coercion does not imply that all members of a coalition adopt identical actions but rather that they coordinate their actions to implement a common strategy. A coalition will have to agree on one outcome that they will advance as the coalition's position. Coalitions can consist of one actor or even be empty. Formally, we have the following:

DEFINITION 1. *A coalition is an ordered pair,* $\mathbf{C} = (x_c, C)$, *such that* $x_c \in Q$ *and* $C \subseteq N$. *A nation, i, is a* member *of the coalition,* $\mathbf{C}$ *(written* $i \in \mathbf{C}$*) iff* $i \in C$. *A coalition,* $\mathbf{C}$, *is* empty *iff* $C = \phi$. *Two coalitions,* $\mathbf{C}_1$ *and* $\mathbf{C}_2$, *are* disjoint *iff* C_1 *and* C_2 *are disjoint. A coalition,* $\mathbf{C}_1$, contains *another coalition,* $\mathbf{C}_2$, *iff* $C_2 \subseteq C_1$ *(written* $\mathbf{C}_2 \subseteq \mathbf{C}_1$*).*

Unlike conventional n-person game theory, each coalition here is associated with its issue position. Because the payoff the members of a coalition expect to receive depends upon the coalition's ability to sustain its issue position in the face of threats from other coalitions and because that ability will change as the coalition's position changes, we cannot divorce a coalition from its position; the same group of actors advancing a different position is a different coalition.

Disjoint coalitions have no members in common, and one coalition *contains* another if all the members of the second coalition are also members of the first coalition.

Conflict arises from the opposition of coalitions. A complete characterization of a conflict requires the specification of the two competing coalitions and the method of coercion used to resolve the conflict. The current state of a conflict is characterized by the members of each coalition, the position that each coalition espouses, and the method of resolution of the crisis. Each slice of a dispute specifying these five quantities (two coalitions, two coalitional positions, and a means of coercion) will be referred to as a *challenge*. A complete *dispute* will be a series of challenges, where each successive challenge responds to the previous challenge through escalation, the recruitment of additional members, or compromise. The formal definition of a challenge is as follows:

DEFINITION 2. *A* challenge *is an ordered triple of a pair of disjoint coalitions and a means of coercion,* $CH = (\mathbf{C}_1, \mathbf{C}_2, \pi)$*, whose first member (the first coalition) is not empty. The first coalition is referred to as the* challenging coalition *and the second as the* countercoalition. *If the countercoalition of a challenge is not empty, then the challenge is* opposed.

Furthermore, there exists a function, f, from the set of all challenges to the set of all probability distributions on Q, $f{:}CH \rightarrow p_Q(x)$ [written $p_{CH}(x)$ for the probability distribution associated with challenge CH)], with the following properties:

a) If CH is unopposed, then $p_{CH}(x) = 1$ for $x = x_{C1}$, and $p_{CH}(x) = 0$ for all other x;

b) If $x \neq tx_{C1} + (1 - t)x_{C2}$ for some $0 \leqslant t \leqslant 1$, then $p_{CH}(x) = 0$; and

c) If $CH = (C_1, C_2, \pi)$ and $CH^* = (\mathbf{C}_1, \mathbf{C}_2, \pi^*)$ and $\pi \geqslant \pi^*$, then $\mathrm{Var}(p_{CH}(x)) \geqslant \mathrm{Var}(p_{CH^*}(x))$.

There also exists a function g from the set of all challenges to the set of n-tuples with non-negative coefficients, $c = (c_1, \ldots, c_i, \ldots, c_n)$, that give the costs of the conflict for each actor, $g{:}CH \rightarrow c$ (written $c_{i,CH}$ for each of the components of the costs associated with a given challenge), with the following properties:

a) If i is not a member of $\mathbf{C}_1$ or a member of $\mathbf{C}_2$, then $c_{i,CH} = 0$; and

b) If $CH = (\mathbf{C}_1, \mathbf{C}_2, \pi)$ and $CH^* = (\mathbf{C}_1, \mathbf{C}_2, \pi^*)$ and $\pi \geqslant \pi^*$, then $c_{i,CH} \geqslant c_{i,CH^*}$ for $1 \leqslant i \leqslant n$.

An actor i's utility for a challenge CH (written $u_i(CH)$) is

$$u_i(CH) = \int_Q p_{CH}(x)\, u_i(x)\, dx - c_{i,CH}. \tag{2}$$

In a given challenge, the two coalitions are labeled as the *challenging coalition* and *countercoalition*, even though the first coalition may not necessarily be challenging the status quo. The f function gives the probability that each different outcome can be imposed by the parties on each other in the absence of a final agreement by the coalitions on the disposition of the issues at hand.[7] The probability distribution associated with each challenge allows the calculation of likely outcomes for that challenge and depends upon the composition and issue positions of the two coalitions and the means of coercion used to resolve the conflict. This distribution is required to have three theoretically desirable properties. First, if a coalition is unopposed, then it can secure its position with certainty. Second, the set of all possible outcomes of a conflict settled solely by coercion are assumed to lie on the line between the contending coalitions' positions, which will be referred to as the *conflict line*. This assumption captures the notion that the use of coercion creates a zero-sum situation across every issue in the dispute and assumes that overall military success allows a coalition to impose its will across all issues equally.[8] Third, given identical coalition memberships and positions, the variance of the distribution increases as the means of coercion intensifies, implying that higher levels of coercion produce a greater spread of outcomes and thus more decisive results than lower levels of coercion.

The g function maps each challenge into the costs that each actor faces from the challenge. These costs include the physical destruction that conflict produces, the political costs that the government faces from waging a war, and the economic costs of war production as those costs are perceived by the political leadership. However, they do not include the costs of changes in the international status quo that are imposed in the resolution of the dispute. The function that specifies the costs must satisfy two constraints: first, any actor not involved in the challenge will incur no costs; second, the costs of all actors cannot decline as the means of coercion increases.

An actor's relative preference for a challenge is given by its expected utility for the consequences of that challenge. The utility attached to each possible outcome is multiplied by that outcome's probability of occurring, and then all of these weighted utilities are summed and the costs of that challenge are subtracted from the total. An actor will prefer one challenge to another if and only if the utility of the former is greater than the utility of the latter.

Conflicts unfold in a sequence of challenges through which the competing coalitions attract support through the addition of members, escalation of the means of coercion when it is to their advantage, and compromise with the opposing coalition when cooperation is more attractive than competition. After a coalition initiates a conflict by making a challenge to the status quo, a coalition opposed to the aims of that coalition may form and attempt to obtain a position of its own or a compromise from the initiating coalition. Of course, this coalition can only form if its formation is preferred to the original challenge by all members of the new coalition. Once there are two opposing coalitions, each will struggle to obtain an advantage over the other coalition. The bargaining between the coalitions will proceed through escalation, recruitment of additional support, and disruption of the other coalition. Each of these bargaining tactics will be reflected by changes in the coalitions' memberships, positions, or adopted means of coercion and will produce a series of shifting challenges that characterize the development of a particular international conflict. Formalizing this notion of shifting coalitions, we have the following:

DEFINITION 3. *One challenge* $CH = (\mathbf{C}_1, \mathbf{C}_2, \pi)$ supercedes *another challenge* $CH^* = (\mathbf{C}_1^*, \mathbf{C}_2^*, \pi^*)$ *iff one of the following conditions holds:*

a) $\pi = \pi^*$, $\mathbf{C}_1^* \subseteq \mathbf{C}_1$, and $\mathbf{C}_1^* \neq \mathbf{C}_1$, for all $i \in \mathbf{C}_1 - \mathbf{C}_1^*$, $u_i(CH) > u_i(CH^*)$, and for all $i \in \mathbf{C}_1^*$, $u_i(CH) \geqslant u_i(CH^*)$;

b) $\pi = \pi^*$, $\mathbf{C}_2^* \subseteq \mathbf{C}_2$, $\mathbf{C}_2^* \neq \mathbf{C}_2$, for all $i \in \mathbf{C}_2 - \mathbf{C}_2^*$, $u_i(CH) > u_i(CH^*)$, and for all $i \in \mathbf{C}_2^*$, $u_i(CH) \geqslant u_i(CH^*)$;

c) $\pi = \pi^*$, $\mathbf{C}_1 = \mathbf{C}_1^* \cup \mathbf{C}_2^*$, and for all $i \in \mathbf{C}_1$, $u_i(CH) \geqslant u_i(CH^*)$;

d) $\pi > \pi^*$, and for all $i \in \mathbf{C}_1$, $u_i(CH) > u_i(CH^*)$.

There are three circumstances under which one challenge can supercede another. The first is when either coalition recruits additional members that find the expanded coalition more attractive than the previous challenge, while the previous members of the expanding coalition value the new challenge at least as much as the previous challenge (conditions a and b above). These new members could be drawn either from the set of nations that are not members of either coalition or from the members of the other coalition.

The second such circumstance is when the coalitions reach a compromise that is at least as attractive to all of their members as the previous challenge (condition c above). In this case, the new challenge will be unopposed, and the issue position of the new challenging coalition will be the compromise between the two coalitions. Including equality in each actor's utility comparison implies that a compromise will be reached if no one objects to it. The carrot of concessions enters the model at this point; presumably, a nation would have to offer concessions on its issue position to get the other coalition to agree to a compromise. Compromises are considered to be enforced by the means of coercion of the superceded challenge to the extent that, should a compromise break down, the means of coercion of the challenge superceding the compromise will have to be at least as high as the means of coercion of the compromise challenge.

The third circumstance under which one challenge can supercede another is when either coalition wishes to escalate the means of coercion in the conflict (condition d above). All the members of the escalating coalition must find the new, escalated challenge preferable to the previous challenge. Some members of a coalition can choose to drop out of an escalating coalition if the remaining members still find escalation without them preferable to the previous challenge with them. Either the challenging or countercoalition can escalate the means of coercion, but the escalating coalition is now treated as the challenging coalition of the new challenge.

International conflict, then, is a series of challenges between coalitions in which each successive challenge supercedes the previous challenge.

DEFINITION 4. *A* dispute *is a finite sequence of challenges,* $(CH_1, CH_2, \ldots, CH_n)$*, such that* CH_{i+1} *supercedes* CH_i *for* $1 \leqslant i \leqslant n - 1$.

A *dispute* is defined as a series of challenges through which the parties to a conflict maneuver for favorable positions by recruiting members to their coalitions, detaching members from the other coalition, and escalating the dispute when such escalation favors them. One challenge in a dispute supercedes another only when it is in the interest of all the members of one coalition to change either the composition of their coalition or their tactics of coercion.

Observed disputes will have to be members of the set of disputes for a given international situation. However, the set of disputes may be very large for any given situation, and the parties to the conflict may not find all disputes strategically desirable. Different solution concepts rule out strategically unsound disputes, limiting the set of possible disputes. The *core*, for example, is the set of all disputes where there does not exist a challenge that supercedes the final challenge of the dispute. By placing a condition on the final challenge, the notion of a core eliminates some disputes from the set of possible disputes. Other solution concepts, like the *competitive solution* (McKelvey, Ordeshook, and Winer, 1978), can be defined in terms of the characteristics of the final challenge of a dispute. Unfortunately, there is no generally acceptable or experimentally supported solution concept for *n*-person games.[9] To demonstrate the type of analysis that can be conducted with the model, the core of the two-nation spatial model will be found. Because only the final challenge of a dispute is needed to judge if the dispute lies in the core, we will simply refer to the *set of core challenges*, with the unstated assumption that the complete core contains all disputes that end in a core challenge.

Before proceeding to the analysis of the two-nation case, three ancillary assumptions about the model will be made. These assumptions were not incorporated in the model formally because they are not essential to its logical structure. They do, however, characterize observed disputes. These assumptions will focus on the probability distribution of the outcomes and on the costs the actors incur for a given challenge. The original structure of the theory places only the weakest constraints on those distributions of outcomes; the ancillary assumptions will flesh out those distributions in an intuitive fashion.

First, the probability distribution of outcomes for a challenge will depend upon only the positions and capabilities of each coalition and the means of coercion used to resolve the conflict. Furthermore, increasing a coalition's capabilities will draw the distribution of outcomes closer to its position. As a coalition grows stronger, it becomes more able to impose its issue position upon the other coalition, and the probability distribution will change to reflect this shift. Formally, this assumption is represented as follows for a coalition C_1:

$$\int_z^{x_{C1}} \frac{\partial p(t)}{\partial cap\ C^1}\, dt > 0 \text{ for all}$$

$$z \in (x_{C1}, x_{C2}). \tag{3}$$

Increasing the capabilities of a coalition shifts the entire probability distribution toward the position of the coalition in the sense that for all possible outcomes z, the chance that the coalition can impose an outcome at least as favorable as z always increases.

The second ancillary assumption is that a coalition's capabilities arise from the contributions of its members to gaining the common goal. Even as it is assumed that nations will contribute all their capabilities to the coalition (eliminating the classic free-rider problem), a coalition's capabilities will not simply be the sum of its members' capabilities for the given means of coercion. A nation's capabilities are extracted from the population of the nation and depend upon the ability of the leadership either to convince or to coerce the population to surrender those resources to the government. Because a nation's utility function reflects the constraints that the populace places on its government's actions, a nation's capabilities in a dispute will depend upon the utility of its coalition's position. As its own coalition's position becomes more attractive to a nation, it should contribute more capabilities to its coalition (all else being equal). Formally, this argument leads to the following assumption for nation i in coalition C_1:

$$\frac{\partial cap\ i}{\partial u_i(x_{C1})} > 0. \tag{4}$$

Each coalition's total capabilities for a given means of coercion will be calculated by summing the contributions of all its members.

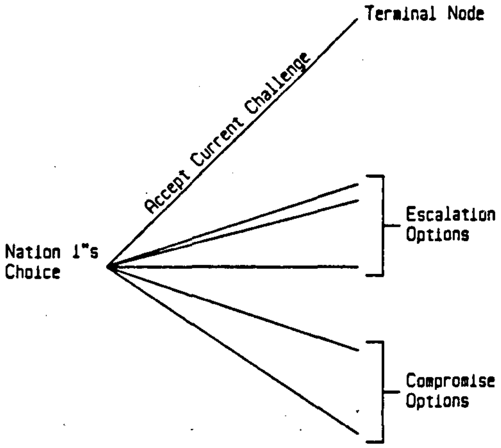
The third ancillary assumption of the model is that the costs each nation suffers in a given challenge are a function only of the composition of the two coalitions and of the means of coercion of the challenge. Under this assumption, nations cannot reduce the costs they face or increase the costs the other side faces by changing positions.

Using the Model: The Core of the Two-Nation Spatial Model

To demonstrate how disputes can be analyzed using the model, the core of the two-nation model will be found. The core here consists of the set of all challenges that cannot be superceded by any other challenge. Core challenges, both those ending in conflict and those ending in compromise, are stable in the sense that neither nation can benefit by escalating the dispute and that there does not exist a compromise solution that both sides prefer to the final challenge. The existence of core challenges for all two-nation disputes will be demonstrated and the character of core challenges examined. Conclusions about the strategies of each side in a dispute, the likelihood of compromise solutions, and the nature of international disputes will be presented.

With only two nations, the model is greatly simplified. First of all, there are no third parties for either nation to recruit into its coalition; consequently, intracoalitional dynamics are eliminated. The set of actions the players can take are then reduced to no action (acceptance of the present challenge), escalation of the means of coercion (unless the dispute is already at the highest level of coercion), and offering or accepting a compromise solution to the dispute (in terms of the

Figure 2. A Decision Node in the Extensive Form Game



model, this corresponds to the formation of a coalition of both nations).

Building an extensive form game tree for the two-nation model produces Figure 2 at each decision node, with the players moving alternately. From any challenge, an actor has a choice of acceptance, escalation, or compromise. Accepting the current challenge produces one new branch of the tree and reaches a terminal node if the other nation has accepted the current challenge in its previous move. Because each resulting challenge is different if any coalition in the challenge adopts a different position, there are an infinite number of escalation and compromise moves possible from any challenge. The game terminates when both nations accept the current challenge (which includes both situations of final compromise and situations in which the dispute is deadlocked because neither escalation nor compromise is possible). The two nations alternate moves, producing the complete game tree.

For a given challenge, each side's expected utility gives the attractiveness of that challenge relative to other challenges for that actor. As previously defined, a nation's expected utility for a challenge is calculated by multiplying the probability of each possible outcome by that nation's utility for that outcome, adding across all possible outcomes, and subtracting that nation's utility for the costs of that challenge. These expected utilities allow us to calculate each nation's preferences over the set of all challenges from the primitive terms of the challenge (position and composition of the coalitions and means of coercion). This comparison can be made even easier because each challenge can be reduced to an equivalent outcome or point for each participant in the dispute (this is established in the first proof in the Appendix). Each challenge then reduces to an equivalent outcome along the line between the two nations' ideal points (equivalent points can be placed on the far side of the other nation's ideal point); for any two challenges, a nation will prefer the challenge with the equivalent point closer to its ideal point.

A nation's equivalent point for a given challenge draws closer to its ideal point as it becomes more risk-acceptant (Morrow, 1985a). As the costs of a given challenge for a nation rise, the equivalent point for the challenge moves away from the nation's ideal point. The effects of escalation on a nation's equivalent point for a challenge (holding each side's position constant) vary depending on whether the nation gains an advantage through escalation and on its risk attitude. If the average outcome (i.e., the mean of the probability distribution) does not shift with the escalation, then escalation will move a nation's equivalent point toward its ideal point if it is risk-acceptant and away from its ideal point if it is risk-averse. This is generally true because escalation increases the riskiness of a challenge, which makes escalation generally attractive to risk-acceptant actors and unattractive to risk-averse actors.

The exact location of the equivalent point of a challenge for an actor will, of course, depend upon the exact composition of the challenge. Even if we fix the membership of a coalition at one of the two actors, it can still alter the challenge and the resulting equivalent points by changing the positions it advances in the challenge. Consequently, one strategic decision the actors face within a given challenge is their choice of position. At first blush, one would suspect that each actor should advance its ideal point as its position; after all, it prefers its ideal point to all other outcomes, and it will reduce its capabilities if it abandons its ideal point to advance a different resolution of the issues, decreasing its chance of gaining favorable outcomes.

This intuition, however, is completely wrong. In general, both actors will adopt positions that are more extreme than their ideal points (see Appendix for a formal discussion of this idea). Specifically, an actor's optimal position lies outside its own ideal point on the line connecting the two actors' ideal points. According to equation (4), an actor's exaggeration of its position reduces its capabilities because it decreases its utility for its position. Nevertheless, actors exaggerating their positions increase their likelihood of gaining very favorable outcomes by expanding the set of favorable outcomes in the conflict set. This increased possibility of gaining very favorable policy outcomes makes each actor willing to run a higher chance of being stuck with unfavorable outcomes. As should be expected, actors will generally exaggerate their positions more as their willingness to take risks increases.[10]

However, at very low levels of coercion, risk-averse actors will actually exaggerate their positions more than risk-acceptant actors will. Because the risk is small at low levels of coercion and because the loss of capabilities is also small for a risk-averse actor, it can justify the gamble of adopting a more extreme position than would be adopted by a comparable risk-acceptant actor. As the dispute escalates, however, the risk-averse actor will moderate its position towards its ideal point to consolidate its strength, while the risk-acceptant actor will exaggerate its position even more. In fact, there is an equilibrium pair of positions for every possible challenge (see Appendix for a proof). This equilibrium insures that the two sides do not get trapped in a cycle of increasingly exaggerated positions.

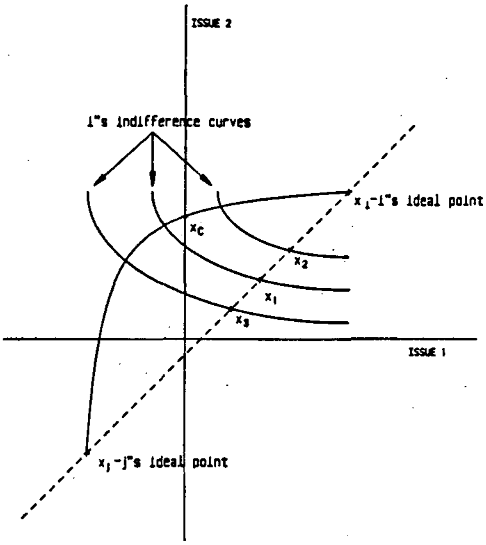
Equivalent points provide a tool for the analysis of conflictual challenges. Central to the analysis of compromise challenges is the Pareto optimal set, the set of all outcomes where any change has to reduce the utility of at least one of the actors. All compromise outcomes should be members of the Pareto optimal set; otherwise, both actors can improve their utility by moving to a point in the Pareto optimal set. Formally, the Pareto optimal set is specified by the following equation:

$$x = [(1-t)x_iA_i + tx_jA_j][(1-t)A_i + tA_j]^{-1}, \quad (5)$$

where x_i and x_j are i's and j's ideal points, A_i and A_j are i's and j's weighting matrices for the relative salience and separability of the issues, and t goes from 0 to 1 (see Appendix for a derivation). The result is a curve from i's ideal point to j's ideal point, the shape of which depends upon each side's weighting of the issues.

If both sides have identical weighting matrices (i.e., they view the issues in exactly the same way), then the Pareto optimal set will be the straight line between their ideal points [set $A_i = A_j = A$ in equation (5)]. When both sides hold the same beliefs about the relative importance of the issues, the set of possible compromises matches the conflict set; that is, the situation is purely conflictual. As the actors begin to differ on the importance of the issues at hand, the Pareto optimal set will bow away from the line between their ideal points. In general then, the Pareto optimal set is a curve between the actors' ideal points, with its

Figure 3. Three Equivalent Points Illustrating Escalation



curvature increasing as the difference in the actors' perceptions of the importance of the issues increases.

To evaluate the attractiveness of one challenge as opposed to another, each actor will compare its equivalent points and will prefer the challenge with the equivalent point closer to its ideal point. The sequence of these equivalent points for a given nation give that nation's preference for escalation. When the sequence of equivalent points approaches its ideal point, a nation will be willing to escalate the dispute; when the sequence moves away from its ideal point, it will not be willing to escalate. Furthermore, the set of compromises that the nation would accept from the current challenge is found by tracing its indifference curve through its equivalent point for the current challenge until that indifference curve intersects the Pareto optimal set; all compromises on the Pareto optimal set between this intersection and the nation's ideal point (inclusive) are at least as good as the current challenge from that nation's perspective.

Figure 3 illustrates this logic; x_1, x_2, and x_3 are nation *i*'s equivalent points for three challenges with escalating means of coercion as the subscript increases. Nation *i* is willing to escalate from the first means of coercion—say political pressure—to the second—perhaps low-level military action—because x_2 is closer to *i*'s ideal point, x_i, than is x_1. On the other hand, nation *i* is unwilling to escalate the means of coercion up to the third level—say openly waged war—because x_3 lies furthest from its ideal point. The set of compromises that nation *i* would accept from the challenge contested through low-level military action is found on the Pareto optimal set as given in Figure 3.

The core of this game is the set of all challenges that cannot be superceded; it is the set of all challenges for which neither side prefers to escalate and for which there does not exist a compromise that both sides prefer to the current challenge. The core tells us whether the dispute will end in conflict or compromise. If it ends in conflict, the core tells us at what level of violence and which side should be expected to win; if it ends in compromise, the core tells us what compromises are feasible.

Core solutions always exist for this game. At the highest means of coercion, either a compromise challenge exists that both sides prefer to a conflictual challenge or else the conflictual challenge with the highest means of coercion cannot be superceded. In the former case, the resulting compromise cannot be superceded because further escalation is not possible and because any change in a compromise on the Pareto optimal set reduces at least one nation's utility for the outcome. In the latter case, the dispute will be resolved through a conflict waged with the highest means of coercion (presumably war). In this case, the two sides cannot find a compromise they both prefer to conflict. Thus, there always exists a core challenge with the highest means of coercion.

Core challenges at lower means of coercion can exist if neither side is willing to escalate the dispute further. These disputes can end in either conflict or compromise, depending upon both sides' evaluations of the value they expect to gain through conflict compared with the attractiveness of compromise.

One problem of obvious interest here is predicting the likelihood of final compromise. What conditions make compromise more likely? There are five aspects to the answer. First, as the nations become less willing to take risks, compromise becomes more likely. As a nation becomes less risk-acceptant, its equivalent point for a given challenge moves further away from its ideal point, increasing the range of compromises it will accept. Of course, we should expect this from the definition of risk-aversion; risk-averse actors prefer the certain choice (i.e., compromise) to the risky choice (i.e., conflict).

Second, as the actors' evaluations of the importance of the issues become increasingly divergent, the Pareto optimal set will bend away from the conflict line. The further the Pareto optimal set bends away from the conflict line, the greater will be the range of pairs of the two sides' risk attitudes that are capable of producing a compromise acceptable to both. As the Pareto optimal set bends outward with the growing divergence of the nation's evaluations of the importance of the issues, there will be pairs of nations that will now find an acceptable compromise that could not find one previously. The possibility of compromise, then, becomes more likely as the divergence of the relative salience of the issues for the actors increases. When the actors disagree about the relative importance of the different issues, compromises are reached more easily because the actors can "trade" the issues off to reach an agreeable compromise, with each actor receiving a favorable outcome on the issue that is more salient to it. Obviously then, the more issues on the negotiating table, the more likely will be the trading of issues that leads to compromise.[11]

Third, escalation can make compromises more stable in some situations. Consider Figure 3 again. From x_1, x_c is an acceptable compromise to the actor because it lies inside the indifference curve through x_1. However, x_2 is preferable to x_c, implying that the actor would be willing to escalate a compromise challenge of position x_c enforced by the first means of coercion. The same compromise outcome, x_c, cannot be superceded if it is enforced by the third means of coercion because further escalation is no longer possible. In other words, escalating from the first to the third means of coercion has stabilized a previously unstable compromise.

Fourth, as the cost of a challenge increases, so does the likelihood of compromise. Increasing the costs moves each side's equivalent point away from its ideal point, thereby increasing the range of acceptable compromises on the Pareto optimal set for that side.

Fifth and finally, in contrast to one-dimensional spatial models of conflict (Morrow, 1985a), compromise in this model may be possible even when both sides disagree about the likely outcome of fighting. When the two sides disagree about which side would prevail in a conflictual dispute, there is a range of outcomes between the two sides' equivalent points for the challenge, which neither side prefers to a conflict. Nevertheless, there can be compromise outcomes that both sides prefer to a conflict because a compromise provides an opportunity to trade control over preferred issues. The conditions of compromises, then, depend closely upon the value each side attaches to the issues in the dispute, the ability of the sides to add issues to the dispute, and their expectations for the value of conflict.

Conclusion

This paper has presented a spatial model of international conflict. The actors are assumed to behave rationally, and their preferences for the resolution of international issues can be represented by a spatial utility function. Their abilities to employ different means of coercion against one another and the desire to form coalitions to enhance their chances of gaining their desired ends lead to a game form that represents their strategic interaction in the dispute. In their attempts to secure their ends, the competing coalitions are capable of recruiting additional members, escalating the means of coercion, or compromising with each other. The model with two nations was analyzed in detail to discover the core of the resulting game and to demonstrate the links between the primitive terms of the theory —that is, the nations' utilities and the probability distributions attached to the challenges—and the decisions that each side will make during the course of the dispute. Conclusions drawn from the model are that nations will generally exaggerate their positions in a dispute for strategic reasons, that core outcomes always exist for this game, and that compromise is facilitated by trading among the different issues in the dispute.

Because the assumptions of the model are quite general, so are its conclusions. Conclusions about specific disputes would, of course, depend upon the specific issues, utility functions, and probability distributions of that dispute. The functions and distributions are the formal representations of how each party values the issues in the dispute and of what is likely to occur if the dispute cannot be resolved peacefully. Empirically, these functions and distributions could be estimated, leading to a direct test of the theory. However, given the paucity of existing data sets focusing on the role of international issues in international conflict, such a direct test seems a distant prospect. Another approach to testing the theory would examine evidence about the conclusions of the theory in historical cases and existing statistical data sets.

Beyond just testing the model, other avenues for further research present themselves. Further deductions from the model are an obvious goal; for example, the three-nation model is informally discussed in Morrow (1985b). There are other directions as well. One advantage of a truly general theory of international conflict is its ability to serve as a building block for testing the logical consistency of traditional theories of international relations. For example, there is currently a great deal of interest in macrohistorical theories of war (e.g., Levy, 1985)—theories that examine the broad scope of international history as it is punctuated by general wars. Such theories attempt to explain both the occurrence and consequences of general wars from a variety of perspectives. The model presented here, if it captures a general theory, must give an account of how certain conflicts flare into general wars and why those general wars appear to occur in cycles over time. The assumptions of the different cyclic theories can be rendered in the terms of the model, and then each macrohistorical theory can be logically tested to see if cycles of general wars can be deduced from its assumptions using the model. Furthermore, the model presented here suggests other intermediate variables, such as the role of issues in cycles of general war, that the literature for the most part has ignored. (Mansbach and Vasquez [1981] discuss issue cycles, but they do not explicitly tie them into the idea of the periodicity of general war.) Only the development of truly general theories of international conflict opens up these possibilities, and while the model presented here is not drawn from the only general theory possible, it is one preliminary path towards such a general theory.

Appendix

This appendix contains the proofs of several statements in the body of the paper. The statements of theorems used in these proofs are drawn from Apostol (1974).

Proof of the Existence of Equivalent Points

A nation's utility for a given challenge, CH, is

$$u_i(CH) = \int_Q p_{CH}(x)\, u_i(x)\, dx - c_{i,CH}, \qquad (6)$$

where Q is just the line segment connecting the positions of the two contending coalitions; if CH is unopposed, then Q is just the position of the coalition, and $u_i(CH) = u_i(x_{C1})$. By the mean-value theorem for integrals, there exists a point x_{i1} such that

$$u_i(x_{i1}) = \int_Q p_{CH}(x)\, u_i(x)\, dx. \qquad (7)$$

Then, because $u_i(x)$ is unbounded on the lower side along the extension of Q, there exists a point, x, with $u_i(x) = u_i(x_{i1}) - c_i(CH)$ by the intermediate value theorem.

Proof That Equilibrium Positions Exist for Any Challenge between Two Actors

We first establish that such equilibrium positions must be found on the line through the two nations' ideal points and then that equilibrium positions do exist along that line. First, consider the case when $A_i = I$; this produces circular indifference curves. For any position off this line—say x'—project the line segment from x' to y, the other side's position, onto the line through x_i and y. We will show that i will prefer to adopt the projection of x', which we will call $x'P$, as its position over x' by comparing the utility of the challenge that results from i's adopting x' as its position, CH, to the utility of the challenge in which i adopts $x'P$ as its position, CH^*.

First, for every point on the line segment from x' to y, its projection has a higher utility than it does because the line of projection will be tangent to the indifference curve through the projection of the point. Then we have the following:

$$\begin{aligned} u_i(CH^*) &= \int_y^{x'P} p_{CH^*}(x)\, u_i(x)\, dx - c_i(CH^*) \\ &> \int_y^{x'} p_{CH^*}(xP)\, u_i(x)\, dx - c_i(CH^*). \qquad (8a) \end{aligned}$$

Second, the projection of x', $x'P$, is closer to x_i than is x'; consequently, the capabilities of i rise, which in turn increases the probability of outcomes close to x_i. Formally, we compare the expected utility given in the second line of equation (8) to the expected utility with $p_{CH}(x)$ substituted for $p_{CH^*}(xP)$:

$$\begin{aligned} &\int_y^{x'} p_{CH^*}(xP)\, u_i(x)\, dx - \int_y^{x'} p_{CH}(x)\, u_i(x)\, dx \\ &= \int_y^{x'} [p_{CH^*}(xP) - p_{CH}(x)]\, u_i(x)\, dx \\ &= \int_y^{x'} [p_{CH^*}(xP) - p_{CH}(x)]\, u_i(x)\, dx \\ &+ \int_y^{z} [p_{CH^*}(xP) - p_{CH}(x)]\, u_i(x)\, dx \qquad (9) \end{aligned}$$

for some $z \in [x', y]$ with $u_i(z) < u_i(x')$. By equation (2), then,

$$\int_z^{x'} [p_{CH^*}(xP) - p_{CH}(x)]\, dx > 0, \qquad (10)$$

and because $\int_y^{x'} [p_{CH^*}(xP) - p_{CH}(x)]\, dx = 0$,

$$\int_y^{z} [p_{CH^*}(xP) - p_{CH}(x)]\, dx < 0. \qquad (11)$$

Returning to the last line of equation (9), we have the following:

$$\begin{aligned} &\int_z^{x'} [p_{CH^*}(xP) - p_{CH}(x)]\, u_i(x)\, dx \\ &+ \int_y^{z} [p_{CH^*}(xP) - p_{CH}(x)]\, u_i(x)\, dx \end{aligned}$$

$$> u_i(z) \int_z^{x'} [p_{CH^*}(xP) - p_{CH}(x)]\, dx$$

$$+ u_i(z) \int_y^{z} [p_{CH^*}(xP) - p_{CH}(x)]\, dx$$

$$= u_i(z) \int_y^{x'} [p_{CH^*}(xP) - p_{CH}(x)]\, dx = 0. \quad (12)$$

Returning to equation (8a), we have the following:

$$u_i(CH^*) > \int_y^{x'} p_{CH^*}(xP)\, u_i(x)\, dx - c_i(CH^*)$$

$$> \int_y^{x'} p_{CH}(x)\, u_i(x)\, dx - c_i(CH^*). \quad (8b)$$

Finally, because the costs each actor suffers from a challenge depend only upon the means of coercion and the composition of the two sides, $c_i(CH^*) = c_i(CH)$.

$$u_i(CH^*) > \int_y^{x'} p_{CH}(x)\, u_i(x)\, dx - c_i(CH^*)$$

$$= \int_y^{x'} p_{CH}(x)\, u_i(x)\, dx - c_i(CH)$$

$$= u_i(CH) \quad (8c)$$

Therefore, any actor with circular indifference curves will prefer $x'P$ to x'. With a general spatial utility function, the linear transformation $A_i P A_i^{-1}$, where P is the projection transformation onto the line through $A_i^{-1}y$ and x_i, produces a preferable position because A_i^{-1} transforms the issue space so that i's indifference curves are circular, at which point the above reasoning holds.

Actors will, then, only adopt positions along the line through x_i and x_j because the above logic holds for both of them. To demonstrate that equilibrium positions along this line always exist, consider one actor's choice of position. For actor i, fix actor j's position at x_{C2} and choose x to maximize i's expected utility:

$$u_i(CH) = \int_Q p_{CH}(x)\, u_i(x)\, dx - c_i(CH)$$

$$= \int_{y_0}^{x_0} p(x,t)\, u_i(t)\, dt - c_i(CH), \quad (13)$$

where x_0 and y_0 are the extreme values of the line through the actors' ideal points (recall that Q is compact, so x_0 and y_0 exist). The function $p(x,t)$ is the probability that point t will occur if x is i's chosen position. Differentiate equation (13) and set it equal to 0 to maximize expected utility:

$$0 = \int_{y_0}^{x_0} \frac{\partial p(x,t)}{\partial x} u_i(t)\, dt. \quad (14)$$

Differentiating $p(x,t)$ with respect to x using the chain rule,

$$\frac{\partial p(x,t)}{\partial x} = \left[\frac{\partial p}{\partial cap_i}\right] \left[\frac{\partial cap_i}{\partial x}\right]$$
$$- (x - x_{C2})^{-2}\, p(x,t). \quad (15)$$

The second term of the above derivative arises to account for the stretching of the available alternatives by moving to a more extreme position. Substituting equation (15) into equation (14), along with some simple rearrangement of terms, produces the following:

$$(x - x_{C2})^{-2} \int_{y_0}^{x_0} p(x,t)\, u_i(x)\, dt$$
$$= \left[\frac{\partial cap_i}{\partial x}\right] \int_{y_0}^{x_0} \left[\frac{\partial p}{\partial cap_i}\right] u_i(t)\, dt. \quad (16)$$

Equation (16) produces the following implicit relationship:

$$0 = (x - x_{C2})^2 \left[\frac{\partial cap_i}{\partial x}\right]$$
$$\left[\int_{y_0}^{x_0} \left[\frac{\partial p}{\partial cap_i}\right] u_i(t)\, dt\right]$$
$$- \int_{y_0}^{x_0} p(x,t)\, u_i(x)\, dt. \quad (17)$$

A solution exists to this equation because for $x = x_{C2}$, the right-hand side is positive; for $x = x_0$, it is negative, and because it is continuous, it must have a zero between those two points. Then, by the implicit function theorem, the function that takes x_{C2} into the zero of equation (17) is a continuous function. Then, by the Brouwer fixed-point theorem, a fixed point in the game of choosing positions exists because each side's choice of position is continuous and the space is compact and convex. Therefore, equilibrium positions exist on the line through the actors' ideal points.

Discussion of the Location of Equilibrium Positions

The left-hand side of equation (16) is negative because u_i is always negative; the second expression on the right-hand side of equation (16) is positive by assumption. Then $\partial cap_i / \partial x$ will be negative. This can only occur when x is moving away from x_i and x_j, which occurs only when i's position is on the far side of x_i.

Definition of the Pareto Optimal Set

The Pareto optimal set can be found by maximizing one actor's utility while holding the other's constant. Mathematically, this reduces to the solution of the following constrained maximization problem:

$$\text{maximize } F(x,\lambda) = -(x - x_j)A_j(x - x_j)^T + \lambda[(x - x_i)A_i(x - x_i)^T - C], \qquad (18)$$

which produces the following Langrangian:

$$A_j(x - x_j)^T - \lambda A_i(x - x_i)^T = 0$$

$$(x - x_i)\,A_i(x - x_i)^T = C. \qquad (19)$$

A solution of the above system of equations is the following (to see this, set $\lambda = t/(1 - t)$ for $0 < t < 1$:

$$x = [(1 - t)x_iA_i + tx_jA_j]\,[(1 - t)A_i + tA_j]^{-1}. \qquad (20)$$

Notes

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1. The type of restriction depends upon the process the group uses to reach decisions and on the set of choices. The best-known example of such restrictions on preferences is the concept of single-peaked preferences in a one-dimensional policy space, which leads to rational decisions under majority rule (Black, 1958). The recent literature on formal group decision theory suggests that coherent group decisions may be induced by structural rules (Shepsle, 1979) or be likely to fall within a range of values reflecting the consensus of the group (McKelvey, 1983).

2. The question of the limits of rational theories as a tool to explain decisions in international policies is essentially empirical. Clearly, there are some policies that are not consistent (Allison, 1972). However, there is also good evidence to suggest that broad strategic decisions that are critical to national security are consistent with previous decisions (Bueno de Mesquita, 1981, 1985; Newman, 1983; Petersen, 1983). The question is reduced to the empirical question of which decisions exhibit rational consistency and which do not, and in the absence of a theory that predicts rational decisions in different situations (such as the theory presented here), a test of the rationality of different decisions is impossible.

3. Although there are many similarities between this model and the arguments of Mansbach and Vasquez (1981), such as a shared focus on issues, what I am defining as an issue corresponds to their notion of a stake. I define an *issue* as one specific *policy in contention, while they see issues as broad* composites of many different *stakes*, which are "objects that are seen as possessing or representing values" (Mansbach and Vasquez, 1981, p. 58).

4. The determination of the relative importance of issues cannot be directly derived from the salience matrix because the scales of the different issues are not known (Davis, Hinich, and Ordeshook, 1970). It may be that 1 unit on one issue is equivalent to 10 units on another issue.

This utility function differs from the spatial utility functions employed in studies of electoral competition in assuming that actors have consistent attitudes towards risk across the range of outcomes. In most spatial models, risk attitudes are represented by an increasing function of the issue distance, where the function induces a reaction to risk across *different ranges. Although the typical approach is* more general, allowing actors to be risk-acceptant over some ranges of values and risk-averse over others, it is less tractable empirically.

5. Some will complain that I have emphasized the stick of coercion at the expense of the carrot of inducements in international politics. This is not so. In the complete model, enticements to reach agree-

ments are included in the structure of the negotiation between contending coalitions.

6. Coalitions should not be confused with alliances. An *alliance* is a formal, written agreement between two or more nations to coordinate their policies (generally military) over a long period of time, while a coalition is simply a temporary agreement between nations to coordinate their strategies and some of their policies for the duration of a dispute.

7. Wittman (1979) presents a rational model of war termination that examines the implications of different coercive strategies on the final agreement reached between two parties. He argues that the type of agreement reached depends upon each side's ability to impose costs above the cost of a settlement on the other side. However, some wars (e.g., the 1967 Arab-Israeli war) do not end with a final agreement but instead grind to a halt because one party has achieved its objectives and the other party is no longer able to impose appreciable costs on it to force it to the bargaining table.

8. In general, it will not be the case that coercion is equally effective over all issues. When this is the case, the set of possible outcomes without compromise will be a path between the two coalitions' positions rather than a straight line between them.

9. A promising solution concept for n-person games may be found within the concepts of *perfect* or *sequential equilibria* (Kreps and Wilson, 1982; Selten, 1975). Those equilibria are defined for extensive form games and, consequently, are not perfectly matched to traditional n-person game theory solutions, which examine only the characteristic function form and not the extensive form of the game.

10. One reader wondered whether some discussion of how rational actors form expectations under limited information (e.g., Kreps and Wilson, 1982) is needed to establish this point about exaggeration of position. Given that the game is played under perfect and complete information, the actors have no problem forming expectations of their opponent's future moves. Regardless of what the other actor does, each actor is better off adopting an exaggerated position; consequently, the question of expectations is moot. The "game" of position selection resembles the classic game of the prisoners' dilemma because each actor's dominant strategy is to exaggerate its position, and such exaggeration creates the possibility of Pareto suboptimal outcomes, such as a final resolution outside both actors' ideal points.

11. This result is an artifact of the two-nation model; with additional nations, adding issues to a dispute may make resolution of the dispute more difficult because it increases the likelihood that uninvolved third parties will intervene in the dispute. It may be the case that some acceptable compromises are not reached in actual disputes because the two sides are unwilling to add the issues needed to complete a compromise, out of fear of provoking the intervention of an uninvolved party.

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LIBERALISM AND WORLD POLITICS

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Building on a growing literature in international political science, I reexamine the traditional liberal claim that governments founded on a respect for individual liberty exercise "restraint" and "peaceful intentions" in their foreign policy. I look at three distinct theoretical traditions of liberalism, attributable to three theorists: Schumpeter, a democratic capitalist whose explanation of liberal pacifism we often invoke; Machiavelli, a classical republican whose glory is an imperialism we often practice; and Kant, a liberal republican whose theory of internationalism best accounts for what we are. Despite the contradictions of liberal pacifism and liberal imperialism, I find, with Kant and other democratic republicans, that liberalism does leave a coherent legacy on foreign affairs. Liberal states are different. They are indeed peaceful. They are also prone to make war. Liberal states have created a separate peace, as Kant argued they would, and have also discovered liberal reasons for aggression, as he feared they might. I conclude by arguing that the differences among liberal pacifism, liberal imperialism, and Kant's internationalism are not arbitrary. They are rooted in differing conceptions of the citizen and the state.

Promoting freedom will produce peace, we have often been told. In a speech before the British Parliament in June of 1982, President Reagan proclaimed that governments founded on a respect for individual liberty exercise "restraint" and "peaceful intentions" in their foreign policy. He then announced a "crusade for freedom" and a "campaign for democratic development" (Reagan, June 9, 1982).

In making these claims the president joined a long list of liberal theorists (and propagandists) and echoed an old argument: the aggressive instincts of authoritarian leaders and totalitarian ruling parties make for war. Liberal states, founded on such individual rights as equality before the law, free speech and other civil liberties, private property, and elected representation are fundamentally against war this argument asserts. When the citizens who bear the burdens of war elect their governments, wars become impossible. Furthermore, citizens appreciate that the benefits of trade can be enjoyed only under conditions of peace. Thus the very existence of liberal states, such as the U.S., Japan, and our European allies, makes for peace.

Building on a growing literature in international political science, I reexamine the liberal claim President Reagan reiterated for us. I look at three distinct theoretical traditions of liberalism, attributable to three theorists: Schumpeter, a brilliant explicator of the liberal pacifism the president invoked; Machiavelli, a classical republican whose glory is an imperialism we often practice; and Kant.

Despite the contradictions of liberal pacifism and liberal imperialism, I find, with Kant and other liberal republicans, that liberalism does leave a coherent legacy on foreign affairs. Liberal states are

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different. They are indeed peaceful, yet they are also prone to make war, as the U.S. and our "freedom fighters" are now doing, not so covertly, against Nicaragua. Liberal states have created a separate peace, as Kant argued they would, and have also discovered liberal reasons for aggression, as he feared they might. I conclude by arguing that the differences among liberal pacifism, liberal imperialism, and Kant's liberal internationalism are not arbitrary but rooted in differing conceptions of the citizen and the state.

Liberal Pacifism

There is no canonical description of liberalism. What we tend to call *liberal* resembles a family portrait of principles and institutions, recognizable by certain characteristics—for example, individual freedom, political participation, private property, and equality of opportunity—that most liberal states share, although none has perfected them all. Joseph Schumpeter clearly fits within this family when he considers the international effects of capitalism and democracy.

Schumpeter's "Sociology of Imperialisms," published in 1919, made a coherent and sustained argument concerning the pacifying (in the sense of nonaggressive) effects of liberal institutions and principles (Schumpeter, 1955; see also Doyle, 1986, pp. 155-59). Unlike some of the earlier liberal theorists who focused on a single feature such as trade (Montesquieu, 1949, vol. 1, bk. 20, chap. 1) or failed to examine critically the arguments they were advancing, Schumpeter saw the interaction of capitalism and democracy as the foundation of liberal pacifism, and he tested his arguments in a sociology of historical imperialisms.

He defines *imperialism* as "an objectless disposition on the part of a state to unlimited forcible expansion" (Schumpeter, 1955, p. 6). Excluding imperialisms that were mere "catchwords" and those that were "object-ful" (e.g., defensive imperialism), he traces the roots of objectless imperialism to three sources, each an atavism. Modern imperialism, according to Schumpeter, resulted from the combined impact of a "war machine," warlike instincts, and export monopolism.

Once necessary, the war machine later developed a life of its own and took control of a state's foreign policy: "Created by the wars that required it, the machine now created the wars it required" (Schumpeter, 1955, p. 25). Thus, Schumpeter tells us that the army of ancient Egypt, created to drive the Hyksos out of Egypt, took over the state and pursued militaristic imperialism. Like the later armies of the courts of absolutist Europe, it fought wars for the sake of glory and booty, for the sake of warriors and monarchs—wars *gratia* warriors.

A warlike disposition, elsewhere called "instinctual elements of bloody primitivism," is the natural ideology of a war machine. It also exists independently; the Persians, says Schumpeter (1955, pp. 25-32), were a warrior nation from the outset.

Under modern capitalism, export monopolists, the third source of modern imperialism, push for imperialist expansion as a way to expand their closed markets. The absolute monarchies were the last clear-cut imperialisms. Nineteenth-century imperialisms merely represent the vestiges of the imperialisms created by Louis XIV and Catherine the Great. Thus, the export monopolists are an atavism of the absolute monarchies, for they depend completely on the tariffs imposed by the monarchs and their militaristic successors for revenue (Schumpeter, 1955, p. 82-83). Without tariffs, monopolies would be eliminated by foreign competition.

Modern (nineteenth century) imperialism, therefore, rests on an atavistic war machine, militaristic attitudes left over from the days of monarchical wars, and export monopolism, which is nothing more than the economic residue of monarchical finance. In the modern era, imperialists gratify their private interests. From the national perspective, their imperialistic wars are objectless.

Schumpeter's theme now emerges. Capitalism and democracy are forces for peace. Indeed, they are antithetical to imperialism. For Schumpeter, the further development of capitalism and democracy means that imperialism will inevitably disappear. He maintains that capitalism produces an unwarlike disposition; its populace is "democratized, individualized, rationalized" (Schumpeter, 1955, p. 68). The people's energies are daily absorbed in production. The disciplines of industry and the market train people in "economic rationalism"; the instability of industrial life necessitates calculation. Capitalism also "individualizes"; "subjective opportunities" replace the "immutable factors" of traditional, hierarchical society. Rational individuals demand democratic governance.

Democratic capitalism leads to peace. As evidence, Schumpeter claims that throughout the capitalist world an opposition has arisen to "war, expansion, cabinet diplomacy"; that contemporary capitalism is associated with peace parties; and that the industrial worker of capitalism is "vigorously anti-imperialist." In addition, he points out that the capitalist world has developed means of preventing war, such as the Hague Court and that the least feudal, most capitalist society—the United States—has demonstrated the least imperialistic tendencies (Schumpeter, 1955, pp. 95–96). An example of the lack of imperialistic tendencies in the U.S., Schumpeter thought, was our leaving over half of Mexico unconquered in the war of 1846–48.

Schumpeter's explanation for liberal pacifism is quite simple: Only war profiteers and military aristocrats gain from wars. No democracy would pursue a minority interest and tolerate the high costs of imperialism. When free trade prevails, "no class" gains from forcible expansion because

> foreign raw materials and food stuffs are as accessible to each nation as though they were in its own territory. Where the cultural backwardness of a region makes normal economic intercourse dependent on colonization it does not matter, assuming free trade, which of the "civilized" nations undertakes the task of colonization. (Schumpeter, 1955, pp. 75–76)

Schumpeter's arguments are difficult to evaluate. In partial tests of quasi-Schumpeterian propositions, Michael Haas (1974, pp. 464–65) discovered a cluster that associates democracy, development, and sustained modernization with peaceful conditions. However, M. Small and J. D. Singer (1976) have discovered that there is no clearly negative correlation between democracy and war in the period 1816–1965—the period that would be central to Schumpeter's argument (see also Wilkenfeld, 1968, Wright, 1942, p. 841).

Later in his career, in *Capitalism, Socialism, and Democracy*, Schumpeter, (1950, pp. 127–28) acknowledged that "almost purely bourgeois commonwealths were often aggressive when it seemed to pay—like the Athenian or the Venetian commonwealths." Yet he stuck to his pacifistic guns, restating the view that capitalist democracy "steadily tells . . . against the use of military force and for peaceful arrangements, even when the balance of pecuniary advantage is clearly on the side of war which, under modern circumstances, is not in general very likely" (Schumpeter, 1950, p. 128).[1] A recent study by R. J. Rummel (1983) of "libertarianism" and international violence is the closest test Schumpeterian pacifism has received. "Free" states (those enjoying political and economic freedom) were shown to have considerably less conflict at or above the level of economic sanctions than "nonfree" states. The free states, the partly free states (including the democratic socialist countries such as Sweden), and the nonfree states accounted for 24%, 26%, and 61%, respectively, of the international violence during the period examined.

These effects are impressive but not conclusive for the Schumpeterian thesis. The data are limited, in this test, to the period 1976 to 1980. It includes, for example, the Russo-Afghan War, the Vietnamese invasion of Cambodia, China's invasion of Vietnam, and Tanzania's invasion of Uganda but just misses the U.S., quasi-covert intervention in Angola (1975) and our not so covert war against Nicaragua (1981-). More importantly, it excludes the cold war period, with its numerous interventions, and the long history of colonial wars (the Boer War, the Spanish-American War, the Mexican Intervention, etc.) that marked the history of liberal, including democratic capitalist, states (Doyle, 1983b; Chan, 1984; Weede, 1984).

The discrepancy between the warlike history of liberal states and Schumpeter's pacifistic expectations highlights three extreme assumptions. First, his "materialistic monism" leaves little room for noneconomic objectives, whether espoused by states or individuals. Neither glory, nor prestige, nor ideological justification, nor the pure power of ruling shapes policy. These nonmaterial goals leave little room for positive-sum gains, such as the comparative advantages of trade. Second, and relatedly, the same is true for his states. The political life of individuals seems to have been homogenized at the same time as the individuals were "rationalized, individualized, and democratized." Citizens—capitalists and workers, rural and urban—seek material welfare. Schumpeter seems to presume that ruling makes no difference. He also presumes that no one is prepared to take those measures (such as stirring up foreign quarrels to preserve a domestic ruling coalition) that enhance one's political power, despite deterimental effects on mass welfare. Third, like domestic politics, world politics are homogenized. Materially monistic and democratically capitalist, all states evolve toward free trade and liberty together. Countries differently constituted seem to disappear from Schumpeter's analysis. "Civilized" nations govern "culturally backward" *regions*. These assumptions are not shared by Machiavelli's theory of liberalism.

Liberal Imperialism

Machiavelli argues, not only that republics are not pacifistic, but that they are the best form of state for imperial expansion. Establishing a republic fit for imperial expansion is, moreover, the best way to guarantee the survival of a state.

Machiavelli's republic is a classical mixed republic. It is not a democracy—which he thought would quickly degenerate into a tyranny—but is characterized by social equality, popular liberty, and political participation (Machiavelli, 1950, bk. 1, chap. 2, p. 112; see also Huliung, 1983, chap. 2; Mansfield, 1970; Pocock, 1975, pp. 198–99; Skinner, 1981, chap. 3). The consuls serve as "kings," the senate as an aristocracy managing the state, and the people in the assembly as the source of strength.

Liberty results from "disunion"—the competition and necessity for compromise required by the division of powers among senate, consuls, and tribunes (the last representing the common people). Liberty also results from the popular veto. The powerful few threaten the rest with tyranny, Machiavelli says, because they seek to dominate. The mass demands not to be dominated, and their veto thus preserves the liberties of the state (Machiavelli, 1950, bk. 1, chap. 5, p. 122). However, since the people and the rulers have different social characters, the people need to be "managed" by the few to avoid having their recklessness overturn or their fecklessness undermine the ability of the state to expand (Machiavelli, 1950, bk. 1, chap. 53, pp. 249–50). Thus the senate and the consuls plan expansion, consult oracles, and employ religion to manage the resources that the energy of the people supplies.

Strength, and then imperial expansion, results from the way liberty encourages increased population and property, which grow when the citizens know their lives and goods are secure from arbitrary seizure. Free citizens equip large armies and provide soldiers who fight for public glory and the common good because these are, in fact, their own (Machiavelli, 1950, bk. 2, chap. 2, pp. 287–90). If you seek the honor of having your state expand, Machiavelli advises, you should organize it as a free and popular republic like Rome, rather than as an aristocratic republic like Sparta or Venice. Expansion thus calls for a free republic.

"Necessity"—political survival—calls for expansion. If a stable aristocratic republic is forced by foreign conflict "to extend her territory, in such a case we shall see her foundations give way and herself quickly brought to ruin"; if, on the other hand, domestic security prevails, "the continued tranquility would enervate her, or provoke internal disensions, which together, or either of them seperately, will apt to prove her ruin" (Machiavelli, 1950, bk. 1, chap. 6, p. 129). Machiavelli therefore believes it is necessary to take the constitution of Rome, rather than that of Sparta or Venice, as our model.

Hence, this belief leads to liberal imperialism. We are lovers of glory, Machiavelli announces. We seek to rule or, at least, to avoid being oppressed. In either case, we want more for ourselves and our states than just material welfare (materialistic monism). Because other states with similar aims thereby threaten us, we prepare ourselves for expansion. Because our fellow citizens threaten us if we do not allow them either to satisfy their ambition or to release their political energies through imperial expansion, we expand.

There is considerable historical evidence for liberal imperialism. Machiavelli's (Polybius's) Rome and Thucydides' Athens both were imperial republics in the Machiavellian sense (Thucydides, 1954, bk. 6). The historical record of numerous U.S. interventions in the postwar period supports Machiavelli's argument (Aron, 1973, chaps. 3–4; Barnet, 1968, chap. 11), but the current record of liberal pacifism, weak as it is, calls some of his insights into question. To the extent that the modern populace actually controls (and thus unbalances) the mixed republic, its diffidence may outweigh elite ("senatorial") aggressiveness.

We can conclude either that (1) liberal pacifism has at least taken over with the further development of capitalist democracy, as Schumpeter predicted it would or that (2) the mixed record of liberalism—pacifism and imperialism—indicates that some liberal states are Schumpeterian democracies while others are Machiavellian republics. Before we accept either conclusion, however, we must consider a third apparent regularity of modern world politics.

Liberal Internationalism

Modern liberalism carries with it two legacies. They do not affect liberal states separately, according to whether they are pacifistic or imperialistic, but simultaneously.

The first of these legacies is the pacification of foreign relations among liberal states.[2] During the nineteenth century, the United States and Great Britain engaged in nearly continual strife; however, after the Reform Act of 1832 defined actual representation as the formal source of the sovereignty of the British parliament, Britain and the United States negotiated their disputes. They negotiated despite, for example, British grievances during the Civil War against the North's blockade of the South, with which Britain had close economic ties. Despite severe Anglo-French colonial rivalry, liberal France and liberal Britain formed an entente against illiberal Germany before World War I. And from 1914 to 1915, Italy, the liberal member of the Triple Alliance with Germany and Austria, chose not to fulfill its obligations under that treaty to support its allies. Instead, Italy joined in an alliance with Britain and France, which prevented it from having to fight other liberal states and then declared war on Germany and Austria. Despite generations of Anglo-American tension and Britain's wartime restrictions on American trade with Germany, the United States leaned toward Britain and France from 1914 to 1917 before entering World War I on their side.

Beginning in the eighteenth century and slowly growing since then, a zone of peace, which Kant called the "pacific federation" or "pacific union," has begun to be established among liberal societies. More than 40 liberal states currently make up the union. Most are in Europe and North America, but they can be found on every continent, as Appendix 1 indicates.

Here the predictions of liberal pacifists (and President Reagan) are borne out: liberal states do exercise peaceful restraint, and a separate peace exists among them. This separate peace provides a solid foundation for the United States' crucial alliances with the liberal powers, e.g., the North Atlantic Treaty Organization and our Japanese alliance. This foundation appears to be impervious to the quarrels with our allies that bedeviled the Carter and Reagan administrations. It also offers the promise of a continuing peace among liberal states, and as the number of liberal states increases, it announces the possibility of global peace this side of the grave or world conquest.

Of course, the probability of the outbreak of war in any given year between any two given states is low. The occurrence of a war between any two adjacent states, considered over a long period of time, would be more probable. The apparent absence of war between liberal states, whether adjacent or not, for almost 200 years thus may have significance. Similar claims cannot be made for feudal, fascist, communist, authoritarian, or totalitarian forms of rule (Doyle, 1983a, pp. 222), nor for pluralistic or merely similar societies. More significant perhaps is that when states are forced to decide on which side of an impending world war they will fight, liberal states all wind up on the same side despite the complexity of the paths that take them there. These characteristics do not prove that the peace among liberals is statistically significant nor that liberalism is the sole valid explanation for the peace.[3] They do suggest that we consider the possibility that liberals have indeed established a separate peace—but only among themselves.

Liberalism also carries with it a second legacy: international "imprudence" (Hume, 1963, pp. 346–47). Peaceful restraint only seems to work in liberals' relations with other liberals. Liberal states have fought numerous wars with nonliberal states. (For a list of international wars since 1816 see Appendix 2.)

Many of these wars have been defensive and thus prudent by necessity. Liberal states have been attacked and threatened by nonliberal states that do not exercise any special restraint in their dealings with the liberal states.

Authoritarian rulers both stimulate and respond to an international political environment in which conflicts of prestige, interest, and pure fear of what other states might do all lead states toward war. War and conquest have thus characterized the careers of many authoritarian rulers and ruling parties, from Louis XIV and Napoleon to Mussolini's fascists, Hitler's Nazis, and Stalin's communists.

Yet we cannot simply blame warfare on the authoritarians or totalitarians, as many of our more enthusiastic politicians would have us do.[4] Most wars arise out of calculations and miscalculations of interest, misunderstandings, and mutual suspicions, such as those that characterized the origins of World War I. However, aggression by the liberal state has also characterized a large number of wars. Both France and Britain fought expansionist colonial wars throughout the nineteenth century. The United States fought a similar war with Mexico from 1846 to 1848, waged a war of annihilation against the American Indians, and intervened militarily against sovereign states many times before and after World War II. Liberal states invade weak nonliberal states and display striking distrust in dealings with powerful nonliberal states (Doyle, 1983b).

Neither realist (statist) nor Marxist theory accounts well for these two legacies. While they can account for aspects of certain periods of international stability (Aron, 1968, pp. 151–54; Russett, 1985), neither the logic of the balance of power nor the logic of international hegemony explains the separate peace maintained for more than 150 years among states sharing one particular form of governance—liberal principles and institutions. Balance-of-power theory expects—indeed is premised upon—flexible arrangements of geostrategic rivalry that include preventive war. Hegemonies wax and wane, but the liberal peace holds. Marxist "ultra-imperialists" expect a form of peaceful rivalry among capitalists, but only liberal capitalists maintain peace. Leninists expect liberal capitalists to be aggressive toward nonliberal states, but they also (and especially) expect them to be imperialistic toward fellow liberal capitalists.

Kant's theory of liberal internationalism helps us understand these two legacies. The importance of Immanuel Kant as a theorist of international ethics has been well appreciated (Armstrong, 1931; Friedrich, 1948; Gallie, 1978, chap. 1; Galston, 1975; Hassner, 1972; Hinsley, 1967, chap. 4; Hoffmann, 1965; Waltz, 1962; Williams, 1983), but Kant also has an important analytical theory of international politics. *Perpetual Peace,* written in 1795 (Kant, 1970, pp. 93–130), helps us understand the interactive nature of international relations. Kant tries to teach us methodologically that we can study neither the systemic relations of states nor the varieties of state behavior in isolation from each other. Substantively, he anticipates for us the ever-widening pacification of a liberal pacific union, explains this pacification, and at the same time suggests why liberal states are not pacific in their relations with nonliberal states. Kant argues that perpetual peace will be guaranteed by the ever-widening acceptance of three "definitive articles" of peace. When all nations have accepted the definitive articles in a metaphorical "treaty" of perpetual peace he asks them to sign, perpetual peace will have been established.

The First Definitive Article requires the civil constitution of the state to be republican. By *republican* Kant means a political society that has solved the problem of combining moral autonomy, individualism, and social order. A private property and market-oriented economy partially addressed that dilemma in the private sphere. The public, or political, sphere was more troubling. His answer was a republic that preserved juridical freedom—the legal equality of citizens as subjects—on the basis of a representative government with a separation of powers. Juridical freedom is preserved because the morally autonomous individual is by means of representation a self-legislator making laws that apply to all citizens equally, including himself or herself. Tyranny is avoided because the individual is subject to laws he or she does not also administer (Kant, *PP*, pp. 99–102; Riley, 1985, chap. 5).[5]

Liberal republics will progressively establish peace among themselves by means of the pacific federation, or union (*foedus pacificum*), described in Kant's Second Definitive Article. The pacific union will establish peace within a federation of free states and securely maintain the rights of each state. The world will not have achieved the "perpetual peace" that provides the ultimate guarantor of republican freedom until "a late stage and after many unsuccessful attempts" (Kant, *UH*, p. 47). At that time, all nations will have learned the lessons of peace through right conceptions of the appropriate constitution, great and sad experience, and good will. Only then will individuals enjoy perfect republican rights or the full guarantee of a global and just peace. In the meantime, the "pacific federation" of liberal republics—"an enduring and gradually expanding federation likely to prevent war"—brings within it more and more republics—despite republican collapses, backsliding, and disastrous wars—creating an ever-expanding separate peace (Kant, *PP*, p. 105).[6] Kant emphasizes that

> it can be shown that this idea of federalism, extending gradually to encompass all states and thus leading to perpetual peace, is practicable and has objective reality. For if by good fortune one powerful and enlightened nation can form a republic (which is by nature inclined to seek peace), this will provide a focal point for federal association among other states. These will join up with the first one, thus securing the freedom of each state in accordance with the idea of international right, and the whole will gradually spread further and further by a series of alliances of this kind. (Kant, *PP* p. 104)

The pacific union is not a single peace treaty ending one war, a world state, nor a state of nations. Kant finds the first insufficient. The second and third are impossible or potentially tyrannical. National sovereignty precludes reliable subservience to a state of nations; a world state destroys the civic freedom on which the development of human capacities rests (Kant, *UH*, p. 50). Although Kant obliquely refers to various classical interstate confederations and modern diplomatic congresses, he develops no systematic organizational embodiment of this treaty and presumably does not find institutionalization necessary (Riley, 1983, chap. 5; Schwarz, 1962, p. 77). He appears to have in mind a mutual nonaggression pact, perhaps a collective security agreement, and the cosmopolitan law set forth in the Third Definitive Article.[7]

The Third Definitive Article establishes a cosmopolitan law to operate in conjunction with the pacific union. The cosmopolitan law "shall be limited to conditions of universal hospitality." In this Kant calls for the recognition of the "right of a foreigner not to be treated with hostility when he arrives on someone else's territory." This "does not extend beyond those conditions which make it possible for them [foreigners] to attempt to enter into relations [commerce] with the native inhabitants" (Kant, *PP*, p. 106). Hospitality does not require extending to foreigners either the right to citizenship or the right to settlement, unless the foreign visitors would perish if they were expelled. Foreign conquest and plunder also find no justification under this right. Hospitality does appear to include the right of access and the obligation of maintaining the opportunity for citizens to exchange goods and ideas without imposing the obligation to trade (a voluntary act in all cases under liberal constitutions).

Perpetual peace, for Kant, is an epistemology, a condition for ethical action, and, most importantly, an explanation of how the "mechanical process of nature visibly exhibits the purposive plan of producing concord among men, even against their will and indeed by means of their very discord" (Kant, *PP*, p. 108; *UH*, pp. 44–45). Understanding history requires an epistemological foundation, for without a teleology, such as the promise of perpetual peace, the complexity of history would overwhelm human understanding (Kant, *UH*, pp. 51–53). Perpetual peace, however, is not merely a heuristic device with which to interpret history. It is guaranteed, Kant explains in the "First Addition" to *Perpetual Peace* ("On the Guarantee of Perpetual Peace"), to result from men fulfilling their ethical duty or, failing that, from a hidden plan.[8] Peace is an ethical duty because it is only under conditions of peace that all men can treat each other as ends, rather than means to an end (Kant, *UH*, p. 50; Murphy, 1970, chap. 3). In order for this duty to be practical, Kant needs, of course, to show that peace is in fact possible. The widespread sentiment of approbation that he saw aroused by the early success of the French revolutionaries showed him that we can indeed be moved by ethical sentiments with a cosmopolitan reach (Kant, *CF*, pp. 181–82; Yovel, 1980, pp. 153–54). This does not mean, however, that perpetual peace is certain ("prophesiable"). Even the scientifically regular course of the planets could be changed by a wayward comet striking them out of orbit. Human freedom requires that we allow for much greater reversals in the course of history. We must, in fact, anticipate the possibility of backsliding and destructive wars—though these will serve to educate nations to the importance of peace (Kant, *UH*, pp. 47–48).

In the end, however, our guarantee of perpetual peace does not rest on ethical conduct. As Kant emphasizes,

> we now come to the essential question regarding the prospect of perpetual peace. What does nature do in relation to the end which man's own reason prescribes to him as a duty, i.e. how does nature help to promote his *moral purpose?* And how does nature guarantee that what man *ought* to do by the laws of his freedom (but does not do) will in fact be done through nature's compulsion, without prejudice to the free agency of man? . . . This does not mean that nature imposes on us a *duty* to do it, for duties can only be imposed by practical reason. On the contrary, nature does it herself, whether we are willing or not: *facta volentem ducunt, nolentem tradunt.* (*PP*, p. 112)

The guarantee thus rests, Kant argues, not on the probable behavior of moral angels, but on that of "devils, so long as they possess understanding" (*PP*, p. 112). In explaining the sources of each of the three definitive articles of the perpetual peace, Kant then tells us how we (as free and intelligent devils) could be motivated by fear, force, and calculated advantage to undertake a course of action whose outcome we could reasonably anticipate to be perpetual peace. Yet while it is possible to conceive of the Kantian road to peace in these terms, Kant himself recognizes and argues that social evolution also makes the conditions of moral behavior less onerous and hence more likely (*CF*, pp. 187–89; Kelly, 1969, pp. 106–13). In tracing the effects of both political and moral development, he builds an account of why liberal states do maintain peace among themselves and of how it will (by implication, has) come about that the pacific union will expand. He also explains how these republics would engage in wars with nonrepublics and therefore suffer the "sad experience" of wars that an ethical policy might have avoided.

The first source of the three definitive articles derives from a political evolution—from a constitutional law. Nature (providence) has seen to it that human beings can live in all the regions where they have been driven to settle by wars. (Kant, who once taught geography, reports on the Lapps, the Samoyeds, the Pescheras.)

"Asocial sociability" draws men together to fulfill needs for security and material welfare as it drives them into conflicts over the distribution and control of social products (Kant, *UH*, p. 44–45; *PP*, pp. 110–11). This violent natural evolution tends towards the liberal peace because "asocial sociability" inevitably leads toward republican governments, and republican governments are a source of the liberal peace.

Republican representation and separation of powers are produced because they are the means by which the state is "organized well" to prepare for and meet foreign threats (by unity) and to tame the ambitions of selfish and aggressive individuals (by authority derived from representation, by general laws, and by nondespotic administration) (Kant, *PP*, pp. 112–13). States that are not organized in this fashion fail. Monarchs thus encourage commerce and private property in order to increase national wealth. They cede rights of representation to their subjects in order to strengthen their political support or to obtain willing grants of tax revenue (Hassner, 1972, pp. 583–86).

Kant shows how republics, once established, lead to peaceful relations. he argues that once the aggressive interests of absolutist monarchies are tamed and the habit of respect for individual rights engrained by republican government, wars would appear as the disaster to the people's welfare that he and the other liberals thought them to be. The fundamental reason is this:

> If, as is inevitability the case under this constitution, the consent of the citizens is required to decide whether or not war should be declared, it is very natural that they will have a great hesitation in embarking on so dangerous an enterprise. For this would mean calling down on themselves all the miseries of war, such as doing the fighting themselves, supplying the costs of the war from their own resources, painfully making good the ensuing devastation, and, as the crowning evil, having to take upon themselves a burden of debts which will embitter peace itself and which can never be paid off on account of the constant threat of new wars. But under a constitution where the subject is not a citizen, and which is therefore not republican, it is the simplest thing in the world to go to war. For the head of state is not a fellow citizen, but the owner of the state, and war will not force him to make the slightest sacrifice so far as his banquets, hunts, pleasure palaces and court festivals are concerned. He can thus decide on war, without any significant reason, as a kind of amusement, and unconcernedly leave it to the diplomatic corps (who are always ready for such pruposes) to justify the war for the sake of propriety. (Kant, *PP*, p. 100)

Yet these domestic republican restraints do not end war. If they did, liberal states would not be warlike, which is far from the case. They do introduce republican caution—Kant's "hesitation"—in place of monarchical caprice. Liberal wars are only fought for popular, liberal purposes. The historical liberal legacy is laden with popular wars fought to promote freedom, to protect private property, or to support liberal allies against nonliberal enemies. Kant's position is ambiguous. He regards these wars as unjust and warns liberals of their susceptibility to them (Kant, *PP*, p. 106). At the same time, Kant argues that each nation "can and ought to" demand that its neighboring nations enter into the pacific union of liberal states (*PP*, p. 102). Thus to see how the pacific union removes the occasion of wars among liberal states and not wars between liberal and nonliberal states, we need to shift our attention from constitutional law to international law, Kant's second source.

Complementing the constitutional guarantee of caution, international law adds a second source for the definitive articles: a guarantee of respect. The separation of nations that asocial sociability encourages is reinforced by the development of separate languages and religions. These further guarantee a world of separate states—an essential condition needed to avoid a "global, soul-less despotism." Yet, at the same time, they also morally integrate liberal states: "as culture grows and men gradually move towards greater agreement over their principles, they lead to mutual understanding and peace" (Kant, *PP*, p. 114). As republics emerge (the first source) and as culture progresses, an understanding of the legitimate rights of all citizens and of all republics comes into play; and this, now that caution characterizes policy, sets up the moral foundations for the liberal peace. Correspondingly, international law highlights the importance of Kantian publicity. Domestically, publicity helps ensure that the officials of republics act according to the principles they profess to hold just and according to the interests of the electors they claim to represent. Internationally, free speech and the effective communication of accurate conceptions of the political life of foreign peoples is essential to establishing and preserving the understanding on which the guarantee of respect depends. Domestically just republics, which rest on consent, then presume foreign republics also to be consensual, just, and therefore deserving of accommodation. The experience of cooperation helps engender further cooperative behavior when the consequences of state policy are unclear but (potentially) mutually beneficial. At the same time, liberal states assume that nonliberal states, which do not rest on free consent, are not just. Because nonliberal governments are in a state of aggression with their own people, their foreign relations become for liberal governments deeply suspect. In short, fellow liberals benefit from a presumption of amity; nonliberals suffer from a presumption of enmity. Both presumptions may be accurate; each, however, may also be self-confirming.

Lastly, cosmopolitan law adds material incentives to moral commitments. The cosmopolitan right to hospitality permits the "spirit of commerce" sooner or later to take hold of every nation, thus impelling states to promote peace and to try to avert war. Liberal economic theory holds that these cosmopolitan ties derive from a cooperative international division of labor and free trade according to comparative advantage. Each economy is said to be better off than it would have been under autarky; each thus acquires an incentive to avoid policies that would lead the other to break these economic ties. Because keeping open markets rests upon the assumption that the next set of transactions will also be determined by prices rather than coercion, a sense of mutual security is vital to avoid security-motivated searches for economic autarky. Thus, avoiding a challenge to another liberal state's security or even enhancing each other's security by means of alliance naturally follows economic interdependence.

A further cosmopolitan source of liberal peace is the international market's removal of difficult decisions of production and distribution from the direct sphere of state policy. A foreign state thus does not appear directly responsible for these outcomes, and states can stand aside from, and to some degree above, these contentious market rivalries and be ready to step in to resolve crises. The interdependence of commerce and the international contacts of state officials help create crosscutting transnational ties that serve as lobbies for mutual accommodation. According to modern liberal scholars, international financiers and transnational and transgovernmental organizations create interests in favor of accommodation. Moreover, their variety has ensured that no single conflict sours an entire relationship by setting off a spiral of reciprocated retaliation (Brzezinski and Huntington, 1963, chap. 9; Keohane and Nye, 1977, chap. 7; Neustadt, 1970; Polanyi, 1944, chaps. 1–2). Conversely, a sense of suspicion, such as that characterizing relations between liberal and nonliberal governments, can lead to restrictions on the range of contacts between societies, and this can increase the prospect that a single conflict will determine an entire relationship.

No single constitutional, international, or cosmopolitan source is alone sufficient, but together (and only together) they plausibly connect the characteristics of liberal polities and economies with sustained liberal peace. Alliances founded on mutual strategic interest among liberal and nonliberal states have been broken; economic ties between liberal and nonliberal states have proven fragile; but the political bonds of liberal rights and interests have proven a remarkably firm foundation for mutual nonaggression. A separate peace exists among liberal states.

In their relations with nonliberal states, however, liberal states have not escaped from the insecurity caused by anarchy in the world political system considered as a whole. Moreover, the very constitutional restraint, international respect for individual rights, and shared commercial interests that establish grounds for peace among liberal states establish grounds for additional conflict in relations between liberal and nonliberal societies.

Conclusion

Kant's liberal internationalism, Machiavelli's liberal imperialism, and Schumpeter's liberal pacifism rest on fundamentally different views of the nature of the human being, the state, and international relations.[9] Schumpeter's humans are rationalized, individualized, and democratized. They are also homogenized, pursuing material interests "monistically." Because their material interests lie in peaceful trade, they and the democratic state that these fellow citizens control are pacifistic. Machiavelli's citizens are splendidly diverse in their goals but fundamentally unequal in them as well, seeking to rule or fearing being dominated. Extending the rule of the dominant elite or avoiding the political collapse of their state, each calls for imperial expansion.

Kant's citizens, too, are diverse in their goals and individualized and rationalized, but most importantly, they are capable of appreciating the moral equality of all individuals and of treating other individuals as ends rather than as means. The Kantian state thus is governed publicly according to law, as a republic. Kant's is the state that solves the problem of governing individualized equals, whether they are the "rational devils" he says we often find ourselves to be or the ethical agents we can and should become. Republics tell us that

> in order to organize a group of rational beings who together require universal laws for their survival, but of whom each separate individual is secretly inclined to exempt himself from them, the constitution must be so designed so that, although the citizens are opposed to one another in their private attitudes, these opposing views may inhibit one another in such a way that the public conduct of the citizens will be the same as if they did not have such evil attitudes. (Kant, *PP*, p. 113)

Unlike Machiavelli's republics, Kant's republics are capable of achieving peace among themselves because they exercise democratic caution and are capable of appreciating the international rights of foreign republics. These international rights of republics derive from the representation of foreign individuals, who are our moral equals. Unlike Schumpeter's capitalist democracies, Kant's republics—including our own—remain in a state of war with nonrepublics. Liberal republics see themselves as threatened by aggression from nonrepublics that are not constrained by representation. Even though wars often cost more than the economic return they generate, liberal republics also are prepared to protect and promote—sometimes forcibly—democracy, private property, and the rights of individuals overseas against nonrepublics, which, because they do not authentically represent the rights of individuals, have no rights to noninterference. These wars may liberate oppressed individuals overseas; they also can generate enormous suffering.

Preserving the legacy of the liberal peace without succumbing to the legacy of liberal imprudence is both a moral and a strategic challenge. The bipolar stability of the international system, and the near certainty of mutual devastation resulting from a nuclear war between the superpowers, have created a "crystal ball effect" that has helped to constrain the tendency toward miscalculation present at the outbreak of so many wars in the past (Carnesale, Doty, Hoffmann, Huntington, Nye, and Sagan, 1983, p. 44; Waltz, 1964). However, this "nuclear peace" appears to be limited to the superpowers. It has not curbed military interventions in the Third World. Moreover, it is subject to a desperate technological race designed to overcome its constraints and to crises that have pushed even the superpowers to the brink of war. We must still reckon with the war fevers and moods of appeasement that have almost alternately swept liberal democracies.

Yet restraining liberal imprudence, whether aggressive or passive, may not be possible without threatening liberal pacification. Improving the strategic acumen of our foreign policy calls for introducing steadier strategic calculations of the national interest in the long run and more flexible responses to changes in the international political environment. Constraining the indiscriminate meddling of our foreign interventions calls for a deeper appreciation of the "particularism of history, culture, and membership" (Walzer, 1983, p. 5), but both the improvement in strategy and the constraint on intervention seem, in turn, to require an executive freed from the restraints of a representative legislature in the management of foreign policy and a political culture indifferent to the universal rights of individuals. These conditions, in their turn, could break the chain of constitutional guarantees, the respect for representative government, and the web of transnational contact that have sustained the pacific union of liberal states.

Perpetual peace, Kant says, is the end point of the hard journey his republics will take. The promise of perpetual peace, the violent lessons of war, and the experience of a partial peace are proof of the need for and the possibility of world peace. They are also the grounds for moral citizens and statesmen to assume the duty of striving for peace.

Appendix 1. Liberal Regimes and the Pacific Union, 1700–1982

Period

18th Century
Swiss Cantons[a]
French Republic, 1790–1795
United States,[a] 1776–
Total = 3

1800–1850
Swiss Confederation
United States
France, 1830–1849
Belgium, 1830–
Great Britain, 1832–
Netherlands, 1848–
Piedmont, 1848–
Denmark, 1849–
Total = 8

1850–1900
Switzerland
United States
Belgium
Great Britain
Netherlands
Piedmont, –1861
Italy, 1861–
Denmark, –1866
Sweden, 1864–
Greece, 1864–
Canada, 1867–
France, 1871–
Argentina, 1880–
Chile, 1891–
Total = 13

1900–1945
Switzerland
United States
Great Britain
Sweden
Canada
Greece, –1911; 1928–1936

Period

1900–1945 (cont.)
Italy, –1922
Belgium, –1940
Netherlands, –1940
Argentina, –1943
France, –1940
Chile, –1924, 1932–
Australia, 1901
Norway, 1905–1940
New Zealand, 1907–
Colombia, 1910–1949
Denmark, 1914–1940
Poland, 1917–1935
Latvia, 1922–1934
Germany, 1918–1932
Austria, 1918–1934
Estonia, 1919–1934
Finland, 1919–
Uruguay, 1919–
Costa Rica, 1919–
Czechoslovakia, 1920–1939
Ireland, 1920–
Mexico, 1928–
Lebanon, 1944–
Total = 29

1945–[b]
Switzerland
United States
Great Britain
Sweden
Canada
Australia
New Zealand
Finland
Ireland
Mexico
Uruguay, –1973
Chile, –1973
Lebanon, –1975

Period

1945– (cont.)
Costa Rica, –1948; 1953–
Iceland, 1944–
France, 1945–
Denmark, 1945
Norway, 1945
Austria, 1945–
Brazil, 1945–1954; 1955–1964
Belgium, 1946–
Luxemburg, 1946–
Netherlands, 1946–
Italy, 1946–
Philippines, 1946–1972
India, 1947–1975, 1977–
Sri Lanka, 1948–1961; 1963–1971; 1978–
Ecuador, 1948–1963; 1979–
Israel, 1949–
West Germany, 1949–
Greece, 1950–1967; 1975–
Peru, 1950–1962; 1963–1968; 1980–
El Salvador, 1950–1961
Turkey, 1950–1960; 1966–1971
Japan, 1951–
Bolivia, 1956–1969; 1982–
Colombia, 1958–
Venezuela, 1959–
Nigeria, 1961–1964; 1979–1984
Jamaica, 1962–
Trinidad and Tobago, 1962–
Senegal, 1963–
Malaysia, 1963–
Botswana, 1966–
Singapore, 1965–
Portugal, 1976–
Spain, 1978–
Dominican Republic, 1978–
Honduras, 1981–
Papua New Guinea, 1982–
Total = 50

Note: I have drawn up this approximate list of "Liberal Regimes" according to the four institutions Kant described as essential: market and private property economies; polities that are externally sovereign; citizens who possess juridical rights; and "republican" (whether republican or parliamentary monarchy), representative government. This latter includes the requirement that the legislative branch have an effective role in public policy and be formally and competitively (either inter- or intra-party) elected. Furthermore, I have taken into account whether male suffrage is wide (i.e., 30%) or, as Kant (*MM*, p. 139) would have had it, open by "achievement" to inhabitants of the national or metropolitan territory (e.g., to poll-tax payers or householders). This list of liberal regimes is thus more inclusive than a list of democratic regimes, or polyarchies (Powell, 1982, p. 5). Other conditions taken into account here are that female suffrage is granted within a generation of its being demanded by an extensive female suffrage movement and that representative government is internally sovereign (e.g., including, and especially over military and foreign affairs) as well as stable (in existence for at least three years). Sources for these data are Banks and Overstreet (1983), Gastil (1985), *The Europa Yearbook, 1985* (1985), Langer (1968), U.K. Foreign and Commonwealth Office (1980), and U.S.

Department of State (1981). Finally, these lists exclude ancient and medieval "republics," since none appears to fit Kant's commitment to liberal individualism (Holmes, 1979).

[a] There are domestic variations within these liberal regimes: Switzerland was liberal only in certain cantons; the United States was liberal only north of the Mason-Dixon line until 1865, when it became liberal throughout.

[b] Selected list, excludes liberal regimes with populations less than one million. These include all states categorized as "free" by Gastil and those "partly free" (four-fifths or more free) states with a more pronounced capitalist orientation.

Appendix 2. International Wars Listed Chronologically

British-Maharattan (1817–1818)
Greek (1821–1828)
Franco-Spanish (1823)
First Anglo-Burmese (1823–1826)
Javanese (1825–1830)
Russo-Persian (1826–1828)
Russo-Turkish (1828–1829)
First Polish (1831)
First Syrian (1831–1832)
Texas (1835–1836)
First British-Afghan (1838–1842)
Second Syrian (1839–1940)
Franco-Algerian (1839–1847)
Peruvian-Bolivian (1841)
First British-Sikh (1845–1846)
Mexican-American (1846–1848)
Austro-Sardinian (1848–1849)
First Schleswig-Holstein (1848–1849)
Hungarian (1848–1849)
Second British-Sikh (1848–1849)
Roman Republic (1849)
La Plata (1851–1852)
First Turco-Montenegran (1852–1853)
Crimean (1853–1856)
Anglo-Persian (1856–1857)
Sepoy (1857–1859)
Second Turco-Montenegran (1858–1859)
Italian Unification (1859)
Spanish-Moroccan (1859–1860)
Italo-Roman (1860)
Italo-Sicilian (1860–1861)
Franco-Mexican (1862–1867)
Ecuadorian-Colombian (1863)
Second Polish (1863–1864)
Spanish-Santo Dominican (1863–1865)
Second Schleswig-Holstein (1864)
Lopez (1864–1870)
Spanish-Chilean (1865–1866)
Seven Weeks (1866)
Ten Years (1868–1878)
Franco-Prussian (1870–1871)
Dutch-Achinese (1873–1878)
Balkan (1875–1877)
Russo-Turkish (1877–1878)
Bosnian (1878)
Second British-Afghan (1878–1880)
Pacific (1879–1883)
British-Zulu (1879)
Franco-Indochinese (1882–1884)
Mahdist (1882–1885)
Sino-French (1884–1885)
Central American (1885)
Serbo-Bulgarian (1885)
Sino-Japanese (1894–1895)
Franco-Madagascan (1894–1895)
Cuban (1895–1898)
Italo-Ethipian (1895–1896)
First Philippine (1896–1898)
Greco-Turkish (1897)
Spanish-American (1898)
Second Phlippine (1899–1902)
Boer (1899–1902)
Boxer Rebellion (1900)
Ilinden (1903)
Russo-Japanese (1904–1905)
Central American (1906)
Central American (1907)
Spanish-Moroccan (1909–1910)
Italo-Turkish (1911–1912)
First Balkan (1912–1913)
Second Balkan (1913)
World War I (1914–1918)
Russian Nationalities (1917–1921)
Russo-Polish (1919–1920)
Hungarian-Allies (1919)
Greco-Turkish (1919–1922)
Riffian (1921–1926)
Druze (1925–1927)
Sino-Soviet (1929)
Manchurian (1931–1933)
Chaco (1932–1935)
Italo-Ethiopian (1935–1936)
Sino-Japanese (1937–1941)
Russo-Hungarian (1956)
Sinai (1956)
Tibetan (1956–1959)
Sino-Indian (1962)
Vietnamese (1965–1975)
Second Kashmir (1965)
Six Day (1967)
Israeli-Egyptian (1969–1970)
Football (1969)

Changkufeng (1938)
Nomohan (1939)
World War II (1939–1945)
Russo-Finnish (1939–1940)
Franco-Thai (1940–1941)
Indonesian (1945–1946)
Indochinese (1945–1954)
Madagascan (1947–1948)
First Kashmir (1947–1949)
Palestine (1948–1949)
Hyderabad (1948)
Korean (1950–1953)
Algerian (1954–1962)
Bangladesh (1971)
Philippine-MNLF (1972–)
Yom Kippur (1973)
Turco-Cypriot (1974)
Ethiopian-Eritrean (1974–)
Vietnamese-Cambodian (1975–)
Timor (1975–)
Saharan (1975–)
Ogaden (1976–)
Ugandan-Tanzanian (1978–1979)
Sino-Vietnamese (1979)
Russo-Afghan (1979–)
Iran-Iraqi (1980–)

Note: This table is taken from Melvin Small and J. David Singer (1982, pp. 79–80). This is a partial list of international wars fought between 1816 and 1980. In Appendices A and B, Small and Singer identify a total of 575 wars during this period, but approximately 159 of them appear to be largely domestic, or civil wars.

This list excludes covert interventions, some of which have been directed by liberal regimes against other liberal regimes—for example, the United States' effort to destabilize the Chilean election and Allende's government. Nonetheless, it is significant that such interventions are not pursued publicly as acknowledged policy. The covert destabilization campaign against Chile is recounted by the Senate Select Committee to Study Governmental Operations with Respect to Intelligence Activities (1975, *Covert Action in Chile, 1963–73*).

Following the argument of this article, this list also excludes civil wars. Civil wars differ from international wars, not in the ferocity of combat, but in the issues that engender them. Two nations that could abide one another as independent neighbors separated by a border might well be the fiercest of enemies if forced to live together in one state, jointly deciding how to raise and spend taxes, choose leaders, and legislate fundamental questions of value. Notwithstanding these differences, no civil wars that I recall upset the argument of liberal pacification.

Notes

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1. He notes that testing this proposition is likely to be very difficult, requiring "detailed historical analysis." However, the bourgeois attitude toward the military, the spirit and manner by which bourgeois societies wage war, and the readiness with which they submit to military rule during a prolonged war are "conclusive in themselves" (Schumpeter, 1950, p. 129).

2. Clarence Streit (1938, pp. 88, 90–92) seems to have been the first to point out (in contemporary foreign relations) the empirical tendency of democracies to maintain peace among themselves, and he made this the foundation of his proposal for a (non-Kantian) federal union of the 15 leading democracies of the 1930s. In a very interesting book, Ferdinand Hermens (1944) explored some of the policy implications of Streit's analysis. D. V. Babst (1972, pp. 55–58) performed a quantitative study of this phenomenon of "democratic peace," and R. J. Rummel (1983) did a similar study of "libertarianism" (in the sense of laissez faire) focusing on the postwar period that drew on an unpublished study (Project No. 48) noted in Appendix 1 of his *Understanding Conflict and War* (1979, p. 386). I use the term *liberal* in a wider, Kantian sense in my discussion of this issue (Doyle, 1983a). In that essay, I survey the period from 1790 to the present and find no war among liberal states.

3. Babst (1972) did make a preliminary test of the significance of the distribution of alliance partners in World War I. He found that the possibility that the actual distribution of alliance partners could have occurred by chance was less than 1% (Babst, 1972, p. 56). However, this assumes that there was an equal possibility that any two nations could have gone to war with each other, and this is a strong assumption. Rummel (1983) has a further discussion

of the issue of statistical significance as it applies to his libertarian thesis.

4. There are serious studies showing that Marxist regimes have higher military spending per capita than non-Marxist regimes (Payne, n.d.), but this should not be interpreted as a sign of the inherent aggressiveness of authoritarian or totalitarian governments or of the inherent and global peacefulness of liberal regimes. Marxist regimes, in particular, represent a minority in the current international system; they are strategically encircled, and due to their lack of domestic legitimacy, they might be said to "suffer" the twin burden of needing defenses against both external and internal enemies. Andreski (1980), moreover, argues that (purely) military dictatorships, due to their domestic fragility, have little incentive to engage in foreign military adventures. According to Walter Clemens (1982, pp. 117–18), the United States intervened in the Third World more than twice as often during the period 1946–1976 as the Soviet Union did in 1946–79. Relatedly, Posen and VanEvera (1980, p. 105; 1983, pp. 86–89) found that the United States devoted one quarter and the Soviet Union one tenth of their defense budgets to forces designed for Third World interventions (where responding to perceived threats would presumably have a less than purely defensive character).

5. All citations from Kant are from *Kant's Political Writings* (Kant, 1970), the H. B. Nisbet translation edited by Hans Reiss. The works discussed and the abbreviations by which they are identified in the text are as follows:

PP *Perpetual Peace* (1795)
UH *The Idea for a Universal History with a Cosmopolitan Purpose* (1784)
CF *The Contest of Faculties* (1798)
MM *The Metaphysics of Morals* (1797)

6. I think Kant meant that the peace would be established among liberal regimes and would expand by ordinary political and legal means as new liberal regimes appeared. By a process of gradual extension the peace would become global and then perpetual; the occasion for wars with nonliberals would disappear as nonliberal regimes disappeared.

7. Kant's *foedus pacificum* is thus neither a *pactum pacis* (a single peace treaty) nor a *civitas gentium* (a world state). He appears to have anticipated something like a less formally institutionalized League of Nations or United Nations. One could argue that in practice, these two institutions worked for liberal states and only for liberal states, but no specifically liberal "pacific union" was institutionalized. Instead, liberal states have behaved for the past 180 years as if such a Kantian pacific union and treaty of perpetual peace had been signed.

8. In the *Metaphysics of Morals* (the *Rechtslehre*) Kant seems to write as if perpetual peace is only an epistemological device and, while an ethical duty, is empirically merely a "pious hope" (*MM*, pp. 164–75)—though even here he finds that the pacific union is not "impracticable" (*MM*, p. 171). In the *Universal History* (*UH*), Kant writes as if the brute force of physical nature drives men toward inevitable peace. Yovel (1980, pp. 168 ff.) argues that from a post-critical (post-*Critique of Judgment*) perspective, *Perpetual Peace* reconciles the two views of history. "Nature" is human-created nature (culture or civilization). Perpetual peace is the "*a priori* of the *a posteriori*"—a critical perspective that then enables us to discern causal, probabilistic patterns in history. Law and the "political technology" of republican constitutionalism are separate from ethical development, but both interdependently lead to perpetual peace—the first through force, fear, and self-interest; the second through progressive enlightenment—and both together lead to perpetual peace through the widening of the circumstances in which engaging in right conduct poses smaller and smaller burdens.

9. For a comparative discussion of the political foundations of Kant's ideas, see Shklar (1984, pp. 232–38).

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ORGANIZING GROUPS FOR COLLECTIVE ACTION

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How can the beneficiaries of collective action be persuaded to contribute the resources (time, energy, money) necessary for the effort to succeed? Rational and selfish players will recognize they can free ride on the successful contributions of others. If the effort is not successful, they will lose a contribution—and be "suckered." Other than relying on altruism, organizers of the group effort can modify incentives so that players are more prepared to contribute. Laboratory experiments offer one way of assessing the effectiveness of various such modifications; we conducted such tests to see how well contributing is promoted by (1) assuring contributors that they will not lose if the group effort fails (a "money-back guarantee") and (2) enforcing contributions if it succeeds ("fair share"). We expect the latter to be more successful because it is "stable," unlike the former, whose success can be undermined by expectations of that success. Three experimental replications demonstrate that the money-back guarantee is no more successful than a standard dilemma, but fair-share requirements increase contributing significantly over that base. Analysis of subjects' expectations about others' behavior offers some support to the hypothesized process undermining the money-back guarantee, but motivational factors must also be taken into account for a full explanation.

The problem of organizing groups for collective action (whether political or otherwise) has, since Olson (1965), been well understood: at least when the group is large, there is a clear disincentive for the potential beneficiaries to contribute the time, money, or other resources necessary for the group effort to be successful, even if all want that effort to be successful. Why should *I* contribute when there is only a trivial chance that my contribution will make a critical difference and when the only other possibilities are that the group effort will be realized without my contribution (in which case I can "free ride") and that it will not be realized if I do contribute (in which case I will be "suckered")?

In more formal terms, the problem is a *social dilemma* (Dawes, 1980): it involves a dominant incentive (do not contribute) associated with a suboptimal equilibrium (the group effort fails). If individuals follow their self-interest, groups confronting such incentives will not attain objectives that all members want.

In natural situations, of course, individuals do not always appear to follow their self-interest, and this impression is supported by experimental studies of social dilemma behavior in which it is possible to specify payoffs with confidence; some incidence of cooperation is normal in such experiments, much more if a period of group discussion is permitted (see, for example, Caldwell, 1976; Dawes, McTavish, and Shaklee, 1977; Edney and Harper, 1978; Jerdee and Rosen, 1974; Orbell, Schwartz-Shea, and Simmons, 1984; Rapoport, 1974; van de Kragt, Orbell, and Dawes, with Braver and Wilson, 1986). To the extent people do not respond to self-interest in these games, the pessimistic prediction from a priori analysis is not borne out, and the side payments Olson discusses (whether "threats" or "bribes") are not required.

In spite of these data—indeed, because the incidence of cooperation (in one-shot games) in the absence of discussion is, characteristically, low—good sense dictates that we search for institutional ways of solving social dilemmas; that is, that we restructure incentives so that selfish individuals are led by consideration of their private interests to contribute to the common interest.[1] Such a restructuring is in the spirit of Adam Smith's (1976) "unseen hand" and of the derived modern arguments in favor of fully specified private property rights and the market as a mechanism for making social decisions. It is also in the spirit of America's founding fathers and of contemporary efforts to design institutions for collective decision making so that selfish individuals playing political games are led to act in the common interest.

Restructuring incentives is what we investigate in the present article. We offer an empirical study of the relative effectiveness of two ways of modifying incentives in step-level dilemmas: (1) ensuring players that they will not lose a contribution if the group effort fails and (2) enforcing a contribution (requiring a "fair share") in the event the group effort succeeds.

The *money back guarantee* device was used by a group of state system faculty members (The Association of Oregon Faculties) wishing to raise money in 1979 to hire a lobbyist at the state legislature. The desired lobbyist—a public good for all faculty members because any pay increases he produced would go to all faculty in the system, not just to those who contributed—required an annual retainer of $30,000 for his work.[2] The Association asked all faculty in the state for contributions, suggesting $36, $60, or $84, depending on salary. The request contained an explicit promise that all money would be returned if less than the $30,000 was raised. The solicitation was successful.

The logic of *enforced contribution* is often present in efforts by apartment dwellers to resist developers who wish to convert their apartment building into a condominium. The developers offer to sell the units at a reduced rate to anyone wishing to vote for conversion prior to a specified deadline. One "contributes" to the apartment dwellers' effort, then, by withstanding the offer. If the effort fails and the conversion proceeds, those who withstood the offer are out of pocket to the extent of the reduced offer; they have to pay the higher rate. However, it is not possible for an apartment dweller to free ride on the restraint of others because if a sufficient number withstand the offer, the conversion won't occur, and the people who voted "for" won't get the private benefit from the sale. Similar logic can exist in union elections in which the choice is between supporting or not supporting an organizing effort and where, if the vote succeeds, "fair share" provisions exist and all members of a bargaining unit must pay their fees.

In natural circumstances, a wide range of uncontrolled factors can influence outcomes one way or the other, and we cannot say—short of the most detailed study—whether success of the group effort hinges on the presence (or absence) of one or the other of these devices. However, the comparative usefulness of the respective organizational devices is an issue susceptible to theoretical and experimental investigation, and the first purpose of the present study is to compare the operation of the two in an experimental context in which factors that might influence performance in natural circumstances are random. We address this purpose in Experiments 1 and 2.

Our design involves a step-level game (Frohlich and Oppenheimer, 1970; Rapoport, 1985; van de Kragt et al., 1983) in which a "bonus," or public good, is provided to all group members in the event some specified number of them make a fixed contribution. In the *full dilemma*—the standard version of this game—individuals have a positive incentive for not contributing if enough others do contribute *or* if too few others contribute to make a contribution useful. In the former case, the individual can "free ride," and in the latter he or she can avoid wasting a contribution. Our institutional modifications involve *half dilemmas* in which one of these incentives is removed. These are the *money-back guarantee half dilemma* and the *enforced contribution* ("fair share") *half dilemma*, respectively. With the money-back guarantee, individuals receive higher payoffs for not contributing if the public good is provided and no more than contributors if it is not. With the enforced contribution, they receive higher payoffs for not contributing if the public good is not provided and no more than contributors if it is.

There is a third possible outcome of others' choices in any step-level dilemma, whether full or half: that the individual's contribution will be "critical," or necessary for provision of the public good; that is, when k contributions are required, $(k - 1)$ others will contribute and the individual's contribution will make the difference.[3] Setting aside this possibility, we note that players in the half dilemmas have a dominating incentive to withhold a contribution, just as do players in the standard game: they will make more by defecting if one outcome happens and no less by defecting if the other does.

At least in the large number case, it does seem reasonable to set aside the possibility of "criticalness." It is, to use our examples, highly unlikely that all (or, indeed, any) contributing faculty or apartment dwellers contributed because they believed their contribution would be critical. Yet this *is* possible, especially in smaller groups, and it is also possible that there are different beliefs about the probability of being critical in the three conditions. A second purpose of this set of experiments, therefore, is to examine the effects of the money-back guarantee and the enforced contribution on the subjective probability of (1) being critical, (2) wasting one's contribution, and (3) being redundant. We address this purpose in Experiment 3, in which we collect such probability judgments. In addition, we will use these probabilities to estimate the expected value of the payoffs for contribution or noncontribution in the standard dilemma and the two half dilemmas. These expected values are not the same in the three conditions, and we will attempt to relate actual choice between contributing and not contributing to these values both within and across conditions.

Predictions

As Coombs (1973) has pointed out, the prisoner's dilemma offers two incentives to defect: the desire to avoid being "suckered" and the desire to capture the "free rider's" payoff. In Coombs' terms, these incentives are *fear* and *greed*, respectively, and they are redundant; either is sufficient to predict defection by itself. We note that our two half dilemmas each retain one of these incentives while eliminating the other. Providing a money-back guarantee offers the individual protection against loss through contributing, while enforcing a contribution should the good be provided means that nothing is to be gained from withholding a contribution—if the good is provided under enforced contribution, a player will end up paying one way or the other. In 1975, Brubaker proposed that defection was more a consequence of desire to avoid loss through contributing than of a desire for gain. If he is correct, we would expect greater group success from the half dilemma that offers a money-back guarantee than from the half dilemma that enforces contribution.

However, a structural difference between the two predicts the opposite. Let us suppose that people given a money-back guarantee believe that such a guarantee works—that is, it will increase the probability that others will contribute. This belief yields an increase in the subjective probability of the goods being obtained without the individual's contribution and, therefore, an increase in the probability that an attempt to free ride will be successful. For example, a faculty member who registered the money-back guarantee might have assumed that its presence would induce enough other faculty members to contribute to make his or her own contribution unnecessary. In short, the money-back guarantee may not "reduce" the dilemma at all. When such a guarantee is given, people may respond to it no differently from the way they respond to the standard dilemma and contribute for the same reasons they contribute in that standard situation (presumably conscience, fellow feelings, magnanimity, etc.) and in the same numbers. *The instability of the money-back guarantee half dilemma may result in its collapse as a device for improving base rates of contributing.*[4]

In contrast, the enforced contribution dilemma is stable. Suppose that people who know they are operating under an enforced contribution rule believe that it will work—that is, it will increase the probability that others will contribute. This belief implies a decrease in the subjective probability that the good will not be provided by others' contributions—it makes group failure and the attendant reason for not contributing less likely. Why *not* contribute under such circumstances?

From this nonmotivational analysis, we predict that the enforced contribution half dilemma will be more effective in raising contributions than will the money-back guarantee half dilemma.

Methodological Overview

Predictions such as these can be readily tested in the laboratory where all other factors that might influence cooperation rates in natural situations are randomized. We do not claim that public goods games created in the laboratory are simulations of any particular naturally occurring situation; we claim only that the payoff matrices confronting subjects *are* public goods matrices. Similarly, we are not particularly interested in absolute levels of cooperation, which can be expected to vary with subject pools and (as we will see) historical periods. Our interest is in differences in cooperation that might exist between experimental conditions of theoretical interest, and we know of no good reasons for predicting an interaction between condition and population pool.

We created seven-person public goods problems under such laboratory circumstances. After they signed the necessary release form, subjects were seated around a large table and were read instructions.

Each was given a $5 promissory note and was told that he or she could contribute that $5 toward a "bonus" of $70 to the group as a whole. This bonus would be distributed equally so that each group member would receive $10. A specified minimal number of contributions was required for the bonus to be distributed; if that number of contributions was not forthcoming, there would be no bonus. Subjects made the decision between contributing and not contributing simultaneously, anonymously, and only once. The latter fact meant there was no possibility of influencing other subjects' behavior in the future, a consideration crucial to some proposed solutions to social dilemma games—notably Axelrod and Hamilton's (1981). Each subject was paid singly and was on the elevator before the next one was paid; this procedure was explained to the subjects in advance so that they knew their choices would be completely unknown to the other participants. If the subject had not contributed the $5 and the bonus was produced, then the net payoff was $15; if the subject had contributed and the bonus was produced, the net payoff was $10; if the subject had not contributed and not enough other subjects had contributed to produce the bonus, the net payoff was $5; and if the subject had contributed and not enough others had, the net payoff was zero. As was emphasized at the outset, there was no deception. Subjects' payoffs varied, as indicated, as a function of their own choices and of the simultaneous choices of the others in the experimental session.

The game described above is a standard step-level public goods game, one that involves no communication, no opportunity for persuasion or coercion, no possibility of side payments or reciprocity, and no social disclosure of individual choices (except to the experimenters). We modified it in two ways: the money-back guarantee half dilemma and the enforced contribution half dilemma.

Under the money-back guarantee half dilemma, subjects were told that if they contributed their $5, but there were not enough other contributions to ensure provision of the bonus, their $5 would be returned. Thus, there was no way in which subjects could lose their contributions. However, they could still free ride on the contributions of others. If a subject withheld his or her contribution and enough others contributed to provide the bonus, the subject would benefit from both the bonus and the original $5, obtaining a total of $15 for participating in the experiment.

Under the enforced contribution half dilemma, all payments were truncated at $10. Should the bonus be provided, no subject could leave the experiment with more than $10. (Truncating payments at $10 is logically equivalent to forcing all to contribute $5 if the bonus is provided.) Thus, there was no opportunity to free ride. When the bonus was provided by others' contributions, subjects who did not contribute left with the same amount of money—$10—as those who had contributed. However, subjects could still lose their contributions and be "suckered." If enough other people did not contribute to produce the bonus, then individuals who did contribute would lose their $5.

The main difference between our experimental dilemmas and the naturally occurring ones discussed earlier is size; with the smaller numbers demanded by experimental logistics, the probability of being critical may be seen as important. Such beliefs will be discussed, evaluated, and analyzed in Experiment 3. The purpose of these first two experiments is to compare the relative efficiency of the two half dilemmas in enhancing cooperation and to test the prediction that enforcing a contribution in the event the good is provided will elicit more voluntary contributing than will providing a money-back guarantee.

Table 1. Experiment 1: Percentage Contributing, Number of Contributors, and the Analysis of Variance and Scheffé Test Results (3 or 7 required)

		Standard Dilemma (1)	Money-Back Guarantee (2)	Enforced Contribution (3)
Percentage contributing		51	61	86
Number of contributors in each group		1, 2, 2, 3, 3, 3, 4, 5, 6, 7	3, 3, 4, 4, 5, 5, 6	4, 5, 5, 6, 6, 6, 7, 7, 7, 7
Analysis of Variance				
	df	*MS*	*F*	*p*
Between conditions	2	14.05	7.41	.003
Within conditions	24	1.90		
Total	26	2.83		
Post hoc Scheffé tests[a]				
	Average difference	Range for $p = .10$	Range for $p = .05$	Range for $p = .01$
(1) vs. (2)	.59	1.52	1.76	2.32
(1) vs. (3)	2.30	1.38	1.59	2.04
(2) vs. (3)	1.71	1.52	1.76	2.32

[a] *Any difference between means larger than the number specified is significant at the level indicated.*

Experiment 1

Subjects were recruited by advertising in the Eugene, Oregon, *Register-Guard* and in student newspapers at the University of Oregon and Utah State University. Subjects participated in the standard dilemma game, the money-back guarantee half dilemma, or the enforced contribution half dilemma. As much care as possible was taken to ensure that subjects in a particular session did not know each other prior to the experiment. In each condition, contributions from any three or more subjects in seven-subject groups were required for the bonus to be provided. The standard dilemma game and the money-back guarantee half dilemma were run in 1979 (forming the core of Simmons's doctoral dissertation; see Simmons, 1980), while the enforced contribution half dilemma was conducted two years later.[5]

All subjects were given the $5 promissory note, after which the appropriate experimental condition was explained in detail. The explanation covered both the logic of the game and the explicit set of monetary payoffs under each condition. Subjects were "quizzed" about the payoffs, and if there was any misunderstanding, the instructions were explained again. The individual payoffs and anonymity were emphasized. *Subjects were not permitted to communicate among themselves; any questions were directed to the experimenter.* In all conditions, subjects made their choices—either to withhold the $5 promissory note or to contribute it—by checking the appropriate box on the "decision form" they had in front of them.

Results of Experiment 1

Table 1 presents the percentage of subjects contributing, the number of contributors, the analysis of variance results, and the results of a *post hoc* Scheffé test using groups as the unit of analysis.[6] The results indicate that there is no significant difference between contribution rates in the standard dilemma and the money-

Table 2. Experiment 2: Percentage Contributing, Number of Contributors, and the Analysis of Variance and Scheffé Test Results (5 of 7 required)

		Standard Dilemma (1)	Money-Back Guarantee (2)	Enforced Contribution (3)
Percentage contributing.		64	65	93
Number of contributors in each group		3, 4, 4, 4, 4, 4, 5, 5, 6, 6	3, 4, 4, 5, 5, 5, 6	5, 6, 6, 6, 7, 7, 7, 7, 7, 7
Analysis of Variance				
	df	*MS*	*F*	*p*
Between conditions	2	12.24	15.69	.00
Within conditions	24	.78		
Total	26	1.66		
Post hoc Scheffé tests[a]				
	Average difference	Range for $p = .10$	Range for $p = .05$	Range for $p = .01$
(1) vs. (2)	.07	.99	1.14	1.46
(1) vs. (3)	2.00	.89	1.03	1.32
(2) vs. (3)	1.93	.99	1.14	1.46

[a] Any difference between means larger than the number specified is significant at the level indicated.

back guarantee half dilemma (51% and 61%, respectively) while there is a significant difference between contribution rates in the standard dilemma and the enforced contribution half dilemma (51% vs. 86%).

Experiment 2

This experiment was identical in all respects to Experiment 1, except that five out of the seven subjects were required to make a contribution in order for the bonus to be provided. By requiring five contributions instead of three, questions of criticalness, deficiency, and redundancy were reversed. For example, whereas two of six others had to contribute for an individual's contribution to be critical in Experiment 1, four of six others in this experiment had to do so.[7]

Results of Experiment 2

The outcomes of Experiment 2 are given in Table 2. Once again, there is no significant difference between contribution rates in the standard dilemma and the money-back guarantee half dilemma (64% and 65%, respectively) while there is a significant difference between contribution rates in the standard dilemma and the enforced contribution half dilemma (64% vs. 93%). Note that the contribution rates are somewhat higher in this experiment (where five contributions are required to activate the bonus) than in the first experiment (where three contributions are required). An analysis of variance in which the main effect is three vs. five required contributions shows that the number required does not produce a significant main effect ($F_{1,48} = 3.32$, $p > .07$) and that its interaction with the three conditions is not significant ($F_{2,48} = .196$, $p > .7$).

As predicted by the structural hypothesis, the enforced contribution half dilemma was superior to the money-back guarantee. In fact, the contribution rate in the money-back guarantee half dilemma was not significantly higher than the rate in the standard dilemma in either experiment while the differences between the enforced contribution half dilemma and the standard dilemma were large and highly reliable. Whether the money-back guarantee works at all is a moot point; across both experiments it yields a cooperation rate of 63%, as opposed to 57% in the standard dilemma. (A rough median split indicates that if these percentages remained constant, the results would be significant if the sample were multiplied by a factor of about 10.)

Experiment 3

What accounts for the 60% cooperation rate in our standard dilemma without any structural modification? One possible explanation is that such base-line contributors were influenced by motivations unrelated to their own monetary payoff—for example, conscience, altruism, and self-esteem. Another explanation, however, derives from the step-level character of our basic design. These contributors might have believed the probability that they were critical to the provision of the bonus (to themselves, as well as to the group) was greater than .5, in which case they would receive a net gain of $5 by contributing, not a net loss of $5, as they would under any other circumstance. The purpose of our third experiment was to distinguish between these two hypotheses by investigating the relationship between subjective probability estimates about others' contributions and a person's own choice, both within and between groups. This approach was suggested by Rapoport (1985), who begins by postulating that in each of these experimental goods situations, each participant estimates three probabilities:[8]

1. p = the probability of being futile—that is, the probability that whatever one does, an insufficient number of others will contribute for the public good to be provided (less than $[m - 1]$ others when m or more are required);
2. p^* = the probability of being critical—that is, the number of others contributing is exactly the number such that the bonus is provided if and only if the individual contributes (exactly $[m - 1]$ others when m or more are required);
3. $(1 - p - p^*)$ = the probability of being redundant—that is, a sufficient number of others will contribute to ensure that the public good will be provided regardless of what the individual chooses (m or more others when m or more are required).

Rapoport works out the expected values for contributing and not contributing. For example, consider the value of cooperation to be $V(c)$ and that of the bonus to be $V(g)$. Letting the subscript 1 refer to the full dilemma condition, we compute the expected value of contributing to be

$$0 \times p_1 + V(g) \times (1 - p_1) = V(g) \times (1 - p_1). \quad (1)$$

That of not contributing is

$$(p_1 + p^*_1) \times V(c) + (1 - p_1 - p^*_1) \times [V(g) + V(c)] = V(c) + (1 - p_1 - p^*_1) \times V(g). \quad (2)$$

The difference is $[p^*_1 \times V(g) - V(c)]$, which is greater than zero only if

$$p^*_1 > V(c)/V(g) = 1/c, \quad (3)$$

where $[c = V(g)/V(c)]$.

Hence, an individual maximizing expected value would choose to contribute if and only if inequality (3) were satisfied. Similar algebra demonstrates that in the money-back guarantee half dilemma, the expected value of contributing is greater than that of not contributing if and only if

$$p^*_2 > (1 - p_2)/c, \tag{4}$$

where the subscript 2 indicates probability estimates for this second situation. Finally, it can be demonstrated that for the third situation—the enforced contribution half dilemma—the expected value of contributing is greater than that of not contributing if and only if

$$p^*_3 > p_3/(c - 1). \tag{5}$$

Rapoport derives the same three conditions. Note first that all three involve an inequality implying that the maximizer of expected value should contribute if and only if p^* is greater than some value. The inequality involves p^* because the situation in which the potential contributor is critical is the only one in which he or she experiences a net gain of $5 by contributing, rather than a net loss of $5 or indifference. Note also that all three inequalities involve only probabilities and the ratio of $V(c)$ and $V(g)$, that is, c, which in our experiments is equal to two. These equations allow qualitative predictions, even though the actual probabilities p, p^*, and $(1 - p - p^*)$ may vary by condition. That is, within each condition we can compare the choice of each individual to his or her estimate of p^* and in conditions 2 and 3, of p.

Method

We replicated Experiment 2 (5 or more contributions out of 7 required to elicit the bonus) with subjects recruited by advertisements in the student newspaper at Utah State University, Logan, Utah, in late November and early December 1984. In addition to asking subjects to make a choice, we asked them each to estimate the probabilities p, p^* and $(1 - p - p^*)$. As before, each subject was assigned to only one condition on a quasi-random basis, and care was taken that friends or acquaintances were not assigned to the same group. The instructions were identical to those of Experiment 2. The probability estimates were elicited by asking subjects to distribute 100 points, as follows:

> For the following questions you must enter a number between 100 and zero. The number indicates how likely you think it is that the specified outcome will occur. These numbers must total to 100. If you are certain that a specific outcome will occur, put 100 in that box and 0 in the others. If you think that each outcome is equally likely, put 33.3 in each box. Any combination is possible, of course, but remember that all numbers must add to 100.

We then asked for answers to the following questions:

1. What is the likelihood of fewer than four others choosing to invest, that is to say, one, two or three other members of the experiment?[9]
2. What is the likelihood of exactly four others choosing to invest?
3. What is the likelihood of more than four others choosing to invest, that is to say, five or all six of the other members of the experiment?

Five groups were run in each of the conditions: standard dilemma, money-back guarantee half dilemma, and enforced contribution half dilemma.

Results of Experiment 3

Table 3 presents the percentage contributing, the number of contributors in each group, and the analysis of variance and a *post hoc* Scheffé test result between conditions. Note that the overall contribution rate is less in 1984 than in 1979–81, significantly so ($F_{1,40} = 15.14$, $p < .0005$). Nevertheless, the pattern is the same: there is no significant difference between the standard dilemma and the money-back guarantee half dilemma, and there is a significant difference between

Table 3. Experiment 3: Percentage Contributing, Number of Contributors, and the Analysis of Variance and Scheffé Test Results (5 of 7 required)

		Standard Dilemma (1)	Money-Back Guarantee (2)	Enforced Contribution (3)
Percentage contributing		23	43	77
Number of contributors in each group		1, 1, 2, 2, 3	5, 2, 1, 3, 4	4, 5, 6, 6, 6
Analysis of Variance				
	df	*MS*	*F*	*p*
Between conditions	2	18.47	15.39	.0005
Within conditions	12	1.20		
Total	14	3.67		
Post hoc Scheffé tests[a]				
	Average difference	Range for $p = .10$	Range for $p = .05$	Range for $p = .01$
(1) vs. (2)	1.40	1.64	1.93	2.58
(1) vs. (3)	3.80	1.64	1.93	2.58
(2) vs. (3)	2.40	1.64	1.93	2.58

[a]Any difference between means larger than the number specified is significant at the level indicated.

the full dilemma and the enforced contribution half dilemma.

As we pointed out earlier, our interest is in behavioral differences between theoretically relevant conditions in experiments such as ours not in absolute levels of cooperation, which can be expected to reflect cultural and other parametric differences. It is easy—no doubt too easy—to speculate on the causes, as well as the consequences, of what here seems to be a behavioral difference between historical periods.

Table 4 gives the average probability estimates, along with their standard deviations, of being futile, being critical, and being redundant in the three conditions. The estimates are broken down into cooperators vs. defectors. A three-way repeated measures analysis of variance on the data of the second part of Table 4 (treating the estimated probabilities of being futile and of being redundant for each individual nested within conditions as the repeated variable) reveals that probability estimates vary by condition ($F_{2,102} = 5.35$, $p = .01$).[10]

The pattern is clear. The probability of redundancy is enhanced and the probability of deficiency is diminished in both half dilemmas by a ratio of 2:1. The judged probability of being critical is unaffected. This pattern of average estimates is consistent with the expectations model of cooperation; it follows that *on the average*, a subject should cooperate in the three conditions if p^* is greater than .50 for equation (3), .37 for equation (4), and .27 for equation (5). These inequalities are progressively easier to satisfy—but note empirically that, on the average again, none is itself easy to satisfy. Note also that, since the p^*s are inherently incomparable, this prediction is "weak."

Moreover, the analysis shows that there is a main effect for cooperation ($F_{1,102} = 26.36$, $p < .001$) with cooperators having higher estimates of being critical or redundant than defectors. More importantly, there is an interaction effect

Table 4. Experiment 3: Average Probability Estimates, with Standard Deviations, of Being Futile, Critical, and Redundant, and Analysis of Variance for Repeated Measures

	Probability of Being Futile $(M < 4)$		Probability of Being Critical $(M = 4)$		Probability of Being Redundant $(M > 4)$	
Experimental Condition and Choice	Average Probability Estimate	Standard Deviation	Average Probability Estimate	Standard Deviation	Average Probability Estimate	Standard Deviation
By condition						
Standard dilemma	.47	.27	.21	.20	.32	.25
Money-back guarantee	.27	.17	.25	.18	.48	.23
Enforced contribution	.27	.22	.24	.15	.50	.26
By choice and condition						
Standard dilemma						
Cooperators $(N = 9)$	.24	.20	.29	.31	.46	.32
Defectors $(N = 26)$	.55	.25	.18	.13	.26	.20
Money-back guarantee						
Cooperators $(N = 15)$	.25	.15	.29	.19	.46	.18
Defectors $(N = 20)$	.28	.19	.21	.16	.50	.28
Enforced contribution						
Cooperators $(N = 27)$	.20	.16	.23	.15	.57	.25
Defectors $(N = 8)$	.51	.23	.25	.16	.24	.08

for cooperation by condition ($F_{2,101} = 5.91$, $p < .001$). Thus, the estimates of cooperators and defectors are differentially affected by the changes from the standard condition to the two half dilemmas. Are these changes compatible, however, with the hypothesis that cooperation or defection are due to differential expectations aroused by the different conditions?

First, the actual numbers do not support the hypothesis. It is specifically the defectors whose expectations should be compatible with not contributing, but under the standard condition, the defectors' average probability estimates of either deficiency or redundancy is .81, as opposed to .70 for the cooperators (who—according to the model—should not cooperate in such circumstances). Under the money-back guarantee condition, the defectors' average probability estimate of being redundant (which makes free riding possible) is virtually identical to that of the cooperators: .50 vs. .46. Only in the enforced contribution condition are the differential directional predictions for cooperators as a group vs. defectors as a group supported. The average probability estimate of the defectors (there were only eight) of being deficient is .51, as opposed to .20 for the cooperators.

When the predicted behavior of each separate individual is compared with his or her actual behavior, directional results are more compatible. Here we computed the phi value of the 2 × 2 contingency between whether the subject did or did not contribute and whether, for that subject, inequality (3), (4), or (5) is satisfied in the appropriate condition. In the standard dilemma, 7 of the 9 contributors are predicted defectors while none of the defectors is a predicted contributor; phi is .42. Under the money-back guarantee condition, 12 of the 15 contributors are predicted defectors and, of the 20 defectors, 3 are predicted cooperators; phi is .09. Under the enforced contribution condition, 13 of the 27 contributors are predicted defectors while 2 of the 8 defectors are predicted contributors; phi is .23. Pooling across the three conditions, only 19 of the 51 contributors "should have" contributed, while 5 of the 54 defectors "should have" done so too; phi is .33 (chi-square = 11.52, $df = 3$, $p < .01$). Thus, the direction of contingency supports the predictions of expectations, but note that a full 63% of the contributors had a negative expected value for contributing. The phi coefficient is, however, much larger in the standard and enforced contribution conditions than in the money-back guarantee, a finding that could follow from the hypothesized instability of the money-back guarantee condition.

A final individual analysis involved the contingency between contributing or not and estimating that such a contribution would result in a net loss of $5 relative to potential payoff. In the standard condition, such a $5 loss occurs if an individual's contribution would be either deficient or redundant. Of the 9 cooperators, 7 believed that they had a better than .50 probability of sustaining such a loss—that is, the sum of each person's estimated probabilities of deficiency and redundancy was greater than .50. In contrast, 25 of the 26 defectors believed that they had a probability of over .50 of sustaining such a loss. Again, the absolute numbers do not support the expectations model, but the direction does, although not significantly so; phi = .29.

Under the money-back guarantee condition, such a $5 net loss occurs only if the individual is redundant—if more than four others also contribute. Of the 15 cooperators, 5 believed that they had a better than .50 chance of sustaining such a loss while 8 of the 20 defectors believed that they did. Here, even the directional contingency is virtually nonexistent; phi = .07.

Under the enforced contribution condition, such a $5 net loss to a contributing individual occurs only if that individual is deficient—if fewer than four others also contribute. Only 1 of 27 cooperators believed that he or she had a better than .50 probability of sustaining such a loss, while 3 of 8 defectors believed that they did. Once more, the absolute numbers do not support the expectations model, but the direction does; phi = .45. We also compared the last two contingencies by means of a Goodman z test for analyzing the interaction term in a 2 × 2 × 2 table.[11] The resulting value was 1.71 ($p <$ .05, one-tailed), but this value must be treated as merely suggestive, due to the *post hoc* nature of the test and because two cells had expected values of less than 5.0 (1.93 and 3.63).

Finally, pooling across all conditions, of 49 subjects who expected a $5 relative loss with probability greater than .50 were they to contribute, only 13 did. In contrast, of 56 who did not estimate the probability of such a relative loss to be greater than .50, 38 cooperated. This contingency is not only strong (phi = .41), but highly significant; chi-square = 17.87 with 3 degrees of freedom, $p < .001$.

We must point out that the analysis based on potential net loss ignores the outcome in which contributing or not leads to the same outcome.[12] Nevertheless, the results of the analysis constitute a clear comparison of the money-back guarantee with the enforced contribution. The enforced contribution results are more compatible with the expectations model than are the money-back guarantee results (again supporting the hypothesis of instability of expectations under the money-back guarantee). Moreover, in the enforced contribution, the contingency between estimation and choice is stronger when the possibility that leads to the same result for contributing and not contributing is ignored.

Discussion

There is no ambiguity whatever about the success of the money-back guarantee device for eliciting contributions compared with the success of the enforced contribution device: the enforced contribution is superior. The pattern of results is stable over variations in the rate of contribution between regions and over a five-year time span. In all three replications it was superior to the standard dilemma at a high level of statistical significance. In two of three, it was significantly superior to the money-back guarantee and was marginally better in the third. In contrast, in no experiment did the money-back guarantee yield results that were superior to those in the standard dilemma at standard levels of significance.

Much of what social science can demonstrate is already "known" by evolved social systems, and we speculate that the frequent use of the enforced contribution or "fair share" device—most notably, of course, in labor unions—is a consequence of long experience with alternative ways of trying to persuade the beneficiaries of group efforts to contribute to those efforts. Institutions that work or that add nothing beyond some base tend to be found out, as do institutions that do not work.

Yet if frequent use suggests that a mechanism has been found to work, it does not, in itself, explain why that mechanism works. We proposed that "fair share," or enforcing a contribution in the event the group effort succeeds, is successful because expectations of its success do not undermine that success—in contrast to the money-back guarantee. However, while the behavioral data supported the prediction, the expectations data (Experiment 3) lend only directional support. There is clearly a contingency between choice and expectation based on probability estimates, but the actual behavior must be influenced by other factors as well. We speculate that one such factor is the relative motivational strengths of Coombs's (1973) *fear* and *greed*.

Earlier, we mentioned Brubaker's (1975) hypothesis that fear of being "suckered" underlays defection, not desire to capture the "free rider's" payoff (*greed*, in Coombs's terms). This hypothesis predicts that a money-back guarantee, which offers the individual protection against losing a contribution, will work better than enforcing a contribution. This means that opportunity for gain through defecting is removed. The failure of the money-back guarantee to increase contributing beyond the level of the standard dilemma suggests that Brubaker is wrong. Fear of loss through contributing is not the critical motivation underlying defection.

On the other hand, the relative success of the enforced contribution is consistent with the hypothesis that desire for gain through defecting is the motivation underlying defection. By hypothesis, enforcing a contribution if a public good is provided works to promote contributing by convincing people that the good will be provided and by removing the opportunity for free riding if it is provided. Free from the fear that the good will not be provided and that their contribution will be lost, people contribute under this rule because they see nothing to be gained by not contributing and, presumably, because they recognize the personal benefits available from the public good. Enforcing contribution should a public good be provided is, by this hypothesis, an institutional modification that is appropriately attuned to widespread, perhaps characteristic, human motivations.

Notes

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1. In our experiments with a dominant incentive to defect of $5, about 30% of the subjects characteristically cooperate. A pessimist, of course, might see such cooperation rates as high.

2. It was, we maintain, a public good for the state, the country, and the world at large.

3. Thus, these step-level games are not strictly social dilemmas, which require a dominant incentive to defect. Note that this absence of a dominant incentive does not preclude their classification in standard terms as "public goods" games. See Frohlich and Oppenheimer (1970) and van de Kragt, Orbell, and Dawes (1985).

4. Of course, there is the possibility of an endless regress here: if an individual is tempted to free ride due to believing the guarantee will work, he or she may infer that the others will also be tempted, which means that the contribution rate will be lower, which makes the guarantee more important, and so on.

5. The distribution of sessions over locations was as follows:

	Utah	Oregon
Standard dilemma	2	8
Money-back guarantee	3	4
Enforced contribution	5	5

6. The test can be found in Scheffé (1960). The computations were executed through the SPSSX one-way module and its Scheffé test option (SPSSX, 1983, p. 458).

7. The distribution of sessions over locations was as follows:

	Utah	Oregon
Standard dilemma	7	3
Money-back guarantee	4	3
Enforced contribution	5	5

8. We wish to acknowledge our debt to Amnon Rapoport for developing this approach and our pleasure that he found these step-level games of sufficient interest to develop his interpretation of the results. Palfrey and Rosenthal (1984) have developed an alternative interpretation based on mixed strategies. While their approach contains some elements identical to Rapoport's (e.g., solving for indifference between contributing and not contributing), the predictions following from it allow so many possible pure and mixed equilibria that we do not elaborate on it here.

9. The typographical error of omitting zero from this statement apparently had no effect; almost all subjects gave estimates for the three possibilities adding to one.

10. That the estimates of being deficient, critical, and redundant are not equal is a trivial finding ($F_{2,102} = 8.60$, $p < .01$). What we report as effects are all, technically, *interactions* with these three estimates.

11. See Goodman (1964).

12. In fact, this analysis is confounded with the earlier analysis of variance of repeated measures, which treated choice as the independent variable and estimates as the dependent one, rather than vice versa. We present both analyses, complete with significance levels, not because we believe in redundant analyses as a general desideratum—quite the opposite—but because each alone is flawed. In this context, we know of no overall best analysis. Certainly, an overall goodness-of-fit statistic, given directional support but striking discrepancy from predictions, would be inappropriate.

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AGENDA CONTROL, COMMITTEE CAPTURE, AND THE DYNAMICS OF INSTITUTIONAL POLITICS

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Most models of agenda control examine dyadic relations —for example, those between a committee and the floor of a legislature. Such relations, however, are always embedded in a larger context, namely, a political environment composed of voters and interest groups. In this paper we model agenda setters (a legislative committee) as decision makers with limited cognitive abilities who adjust over time to their larger political environment. The legislators' policy positions are endogenous, reflecting the relative strengths of voters wielding the district-specific resource of votes and of interest groups wielding the transferable resource of money. The resulting outcomes indicate that neoclassical models of voting and pluralist models of group influence have each told part of the story. When only votes matter, our boundedly rational agents grope toward equilibria close to those of neoclassical models; however, when mobile resources matter as well, the outcomes depart systematically from those of previous models. In particular, interest groups can make themselves worse off by capturing the committee. The results suggest that agenda control is less powerful than conventionally believed and point toward conditions shaping its effectiveness—conditions highlighting the distinctive contributions of pluralist and neoclassical thinking to a broader theory of political institutions.

Recent years have witnessed important developments in the formal analysis of political institutions. Formal theorists, long centrally concerned with voting and markets, have taken an active interest in questions of structure, hierarchy, and control. Students of organization and public administration, traditionally skeptical of mathematical approaches, have increasingly recognized the relevance of these efforts for their own areas of substantive concern. Encouraged by new perspectives within both groups, formal theories of legislative and bureaucratic behavior have developed rapidly and assumed a larger, more influential role in the institutional literature.

The take-off point was a long time in coming. For decades, institutional analysis was almost entirely sociological and historical in method, with little attention to formal theories or economic modes of analysis. There was one important exception: the Simon-March behavioral tradition, with its emphasis on bounded rationality, adaptation, and dynamic models of organizational decision making. Yet, while this tradition had, and continues to have, substantial

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influence in the realm of ideas, its formal models did little to bring about a methodological reorientation or a wider appreciation of the promise of formal approaches. These changes were set in motion only recently through significant developments in social choice theory and the economics of organization. In social choice theory, new research has moved away from a focus on pure majority rule and its associated disequilibria toward the investigation of institutional mechanisms—agenda control most prominent among them—that shape and stabilize voting outcomes (Denzau and Mackay, 1983; Enelow and Hinich, 1984a, Ferejohn et al, 1984; McKelvey, 1976; Romer and Rosenthal, 1978; Shepsle, 1979; Shepsle and Weingast, 1981). In the economics of organization, economists have moved to provide an organizational foundation for neoclassicism, putting rational choice theory to use in explaining the emergence and behavior of complex organizations, the nature of hierarchic control, and its function as a productive alternative to markets (Alchian and Demsetz, 1972; Coase, 1937; Hess, 1983; Moe, 1984; Williamson, 1975).

These research programs may well transform our understanding of institutions. In particular, we agree with Riker (1982) that the new understanding of agenda control is a major insight that highlights the political significance of institutional structure. The existence of structure-induced equilibria (Shepsle, 1979)—of policies that are selected because well-positioned decision makers manipulate the choice sets of other agents—indicates that political institutions are not merely transmission belts for environmental forces. Instead, they shape outcomes in patterned ways.

The idea that manipulated choice sets can stabilize policy outcomes is a powerful and fertile concept, one that has created a research program within the larger stream of formal analyses of political organizations. An indicator that an idea has created a research program is the number of its progeny. By this criterion, the concept of institutionally based agenda control is well on its way.[1] Because many of these works draw inspiration from the large and growing literature on the economics of information and organization, it is safe to say that the neoclassical analysis of agenda control—and, more generally, of political institutions—will be a fruitful research program for many years to come.

Yet much remains to be done. As part of a pioneering effort to gain an analytical foothold, current models are usually kept quite simple, abstracting from much of the substantive and theoretical features that the institutional literature has long emphasized and, in fact, that formal modelers themselves often recognize as integral components of future, more comprehensive models (see, e.g., Shepsle, 1983). It is impossible at this point to know with any certainty which features are essential and which can safely be overlooked, but it does seem clear that the theoretical progress hinges on systematic efforts to explore these avenues and thus to elaborate existing models by tentatively incorporating what now appear to be important aspects of institutional detail and behavior. Several dimensions stand out as particularly promising candidates.

1. *Institutional context.* Most models focus on relationships that are essentially two sided: chairman-committee, committee-legislature, bureau-committee, principal-agent. Yet as the empirical literature has long emphasized, it is difficult to understand these relationships without placing them in larger context. In the United States especially, bureaus, the legislature, committees, and constituents are all bound up with one another. Their incentives and interactions are mutually structured, and they should presumably be modeled as such.

2. *Endogenous policy positions.* Institu-

tional models of agenda setting often take the policy positions of political actors as exogenously fixed when, in fact, these positions often derive from dynamic relations among actors and change in systematic ways over time. Legislators motivated by reelection, for example, have induced preferences over policy alternatives as a reflection of constituency influences, and their induced preferences change with changes in the constituency. In a larger institutional model, legislators' policy positions would not be taken as given but would become endogenous components of the system.

3. *Political resources.* Most models assume that the only political resource influencing politicians is voting, a resource that cannot flow across districts. Yet in electoral campaigns the significance of money—the quintessentially mobile resource—is evident to scholars and journalists alike (Drew, 1983; Jacobson, 1980). A more elaborate model would recognize that legislators' policy positions may respond in varying degrees to both district-specific and transferable resources.

4. *Cognitive limitations.* While the most common modeling assumption is that people are computationally flawless, the thrust of the empirical literature is that the patterns of behavior we observe in the policy process are explained in large part by the cognitive limitations of decision makers. These behavioral patterns, moreover, can play an important role in compensating for the imperfections of individuals, allowing them to generate collective outcomes that reflect the operation of a higher level of "intelligence." None of this can be investigated or explained if cognitive limitations are assumed away at the outset.

5. *Process.* Current models use comparative statics as the main method of analysis. Comparative static predictions are invaluable, but they do not address the process of interaction, how it evolves over time, or how equilibria are determined. Particularly in a world of cognitive limitations and endogenous policy positions, insight into this process would appear to be crucial to an understanding of institutions and their outcomes.

In this paper, we attempt to move along all five of these dimensions in offering a new perspective on agenda control and its consequences for institutional outcomes. We begin with a model of institutional politics that (1) incorporates a bureau, a majority-rule legislature, and constituency groups, (2) makes legislative policy positions endogenous, (3) assumes political resources can flow freely throughout the system, (4) views all actors as having limited information and cognitive abilities, and (5) generates an institutional process of mutually adaptive adjustment among these actors over time. In an earlier article, we developed this model in some detail and explored its basic properties (Bendor and Moe, 1985). Its hallmark is that, under quite general conditions, the system moves toward what we have called a *pluralist equilibrium*—a steady state characterized by a balance of interest group resources.

Here, we use this earlier institutional model as a foundation, and we explore agenda control by introducing two new components: (1) a legislative committee with monopoly power over the proposal of policy alternatives and (2) a system of electoral districts whose boundaries constrain the flow of political resources. The general question then becomes, how does the known behavior of the institutional system change in response to these new components, and how do these impacts vary with changes in the fundamental structure of the system itself?

More concretely, we will be investigating ideas that have long held sway in the institutional literature and, to this point, have generally been reinforced by formal analysis. The conventional wisdom is that oversight committees tend also to be "high-demand" committees—that is, committees in some sense "captured" by interest groups benefitting from the policies in question—and that the committees are in fact successful in using their agenda control powers to bring about the policy outcomes preferred by the capturing groups. However accurate this may be as a description of politics, its analytical foundations remain unclear. Are agenda powers in themselves sufficient to guarantee committee success in engineering policy outcomes, or is capture somehow integral or necessary to their efficacious use? Does capture of an agenda-setting committe in fact work to the advantage of the capturing group? How are agenda control, committee capture, and the relationship between them conditioned by the mobility of political resources and the constraining role of electoral districts?

Most generally, our analyses will show that agenda control and committee capture are more problematic than the literature tends to suggest. The mobility of political resources plays a crucial role in this regard. When resources are district specific, for example, successful committee capture and agenda control are promoted in much the same fashion that now-familiar spatial models lead us to expect, and this occurs despite the cognitive limitations of our actors. When all political resources can flow across districts, however, very different patterns emerge, patterns consistent with pluralist images of balance of power policy making. With mobile resources, agenda control is often rendered impotent as committee members move to the legislative center, and efforts at capture not only may fail in the long run but may even leave the intended beneficiary worse off than if these mechanisms of institutional power had never been used in the first place. Because in the real world of American politics some resources are district specific and some are mobile, it seems clear that theories of agenda control and institutional politics must incorporate both pluralist and spatial notions of political influence.

This work is an effort to shed new light on agenda control and its implications for institutional politics, but we also want to encourage some rethinking on the methodological front. As formal models go, ours is a bit unorthodox—it is a computer simulation anchored in the Simon-March behavioral tradition and, to our knowledge, is the only analysis of agenda control to derive from this line of work. Our hope is that while neoclassical methods have recently been instrumental in promoting the formal analysis of institutions, efforts to build more comprehensive models will recognize the value of this tradition and put its ideas and methods to effective use.

An Overview of the Model

Our model of institutional politics consists of three basic types of participants—an agency, legislators, and interest groups—and the interactions among them. The detailed assumptions governing their motivation, information, and adaptive adjustments have been described elsewhere (Bendor and Moe, 1985), and, for economy of presentation, we will not cover the same ground here. Readers unfamiliar with the model are provided with a summary of assumptions in the Appendix.

At a general level, the model is quite simple. The electorate is comprised of two groups of citizens, one that benefits from agency programs and one that is hurt by them. These citizens express their preferences about agency activity through the electoral connection: they evaluate legislators based on the legislators' positions on agency budgets. Generally speaking, citizens who benefit from higher levels of agency activity support higher agency budgets and thus legislators who vote accordingly; citizens who shoulder costs as a result of agency activism do the reverse. Legislators adjust their budgetary votes over time in an effort to enhance their electoral security. These votes, aggregated through the legislature's rules and procedures, yield budgets that are passed along to the agency, enabling it to generate some level of programmatic activity.[2] How much activity a given budget generates is largely up to the agency, however, and depends on what the agency values—budgets, slack, policy, or some combination of these—along with its assessment of whether it is being rewarded or sanctioned for its past choices of activity level. Seeking to enhance its own utility, the agency decides how much activity to produce (through its choice of how "efficiently" to translate its budget into output), and this new level of activity is then experienced in the form of new levels of benefits and costs for citizens. This alters the stakes of political activity for citizens, prompting them to adjust the amount of resources they devote to politics. Having done so, they turn to a fresh evaluation of their legislators, noting how the legislators' votes changed over the period, and adjust their support and opposition accordingly among them. This sends the cycle of influence into another iteration—stimulating new changes in legislative votes, agency activity, political resources, and so on until an equilibrium is eventually reached at which budgets, activity levels, and other systemic outcomes take on stable values.

The model is not specialized as to types of policies or agencies, but because it is often helpful to think in more concrete terms, we will develop the analysis with reference to an agency engaged in consumer protection regulation. We take the relevant groups of citizens to be "consumers" and "business firms." The agency's output is the level of regulatory enforcement, and stronger enforcement is assumed to benefit consumers and to impose costs on firms.

The analysis is organized by the presence or absence of the two components we ultimately want to graft onto the basic institutional model: an agenda-controlling committee and electoral districts. First we assume that all relevant resources are mobile throughout the system—that is, there are no districts—and explore the consequences of introducing a committee with agenda-setting powers. In the next section we go to the other extreme, exploring the role of such a committee when the political world exhibits electoral districts and all relevant resources are constrained from flowing across district boundaries.[3] Finally, we combine the two approaches by investigating agenda control when both mobile and district-specific resources come into play.[4]

Mobile Resources

In this section, all political resources are assumed to flow freely throughout the system, generated by firms and consumers in response to the prevailing level of regulatory enforcement. As enforcement rises, the political stakes—benefits for consumers, costs for business—rise as well, and both sectors devote more resources to the electoral process. These political "warchests" are then divided among legislators in accordance with their positions on agency budgets. The larger the budgetary increase advocated by a given legislator, the more support he will attract from consumers and the more opposition from business; the converse obtains, of course, for a decrease in the budget.

From Majority Rule to Agenda Control

Suppose we take as our null model—call it Version 1-1 (model 1, version 1)—a context in which the bureau is a budget

Table 1. Computer Runs of Model 1: Mobile Resources

	Simulation Version					
	1	2	3	4	5	6
Parameters						
Committee has agenda authority?	no	yes	no	yes	yes	yes
Interest groups allocate extra resources to committee?	neither	neither	consumers	consumers	consumers	consumers and firms
Initial budget and enforcement	low	low	low	low	high	high
Outcomes						
Enforcement	169	172	145	151	242	177
Efficiency	.53	.60	.52	.76	.81	.75
Type of equilibrium[a]	PE	PE	MPE	SE	SE	PE

[a] PE = pluralist equilibrium
MPE = modified pluralist equilibrium
SE = structural equilibrium

maximizer overseen by the pro-consumer contingent within the legislature. This context would certainly appear to be favorable from the standpoint of consumer interests. While the bureau does not care at all about pursuing consumer enforcement objectives, it does desire bigger budgets and is presumably willing to do whatever it takes to get them. Consumerist legislators are supportive of budgetary increases for the agency because, other things being equal, these increases translate into higher levels of enforcement. If the bureau should behave perversely with its larger budgets, moreover, legislators have oversight mechanisms to bring the bureau back into line.

Now suppose we initialize this model—which still has no committee—by assuming that the bureau begins with a budget of 100 units and an efficiency of .8, which yield an initial enforcement level of 80. This level of enforcement is well below the social optimum of 150. Can consumers and their legislative agents succeed in increasing bureaucratic enforcement?

The results, set out in Table 1, indicate that enforcement does go up. The system reaches equilibrium beyond the social optimum at an enforcement level of 169. This is a *pluralist equilibrium*—that is, the point at which the resources contributed to politics by business and consumers are equal and thus at which a balance of forces exists in the constituency. The reason the system arrives at such an equilibrium, however, is not that consumers and their supporters in the legislature are "powerful," for this result (which we could easily demonstrate with additional runs of the model) is in fact independent of the suboptimal starting point. Had we started the system out at an enforcement level well beyond the social optimum, it would then have moved down to virtually the identical pluralist equilibrum—consumers would have lost ground rather than gained it. This model of a budget-maximizing bureau constrained by oversight, then, is hardly a setup for consumer power. Its dynamics "naturally" move it to a pluralist equilibrium around an enforcement level of 169, regardless of where the system starts out.

Now let's introduce a committee into the legislature. Specifically, let's assume the committee has the authority each period to pose one-time, take-it-or-leave-it budgets to the full legislature under a closed rule, with failure of the committee's proposal resulting in automatic adoption of the prevailing status quo budget. Legislative decision making proceeds as follows. The committee's most desired budget is determined by a vote of its members, with the median member being decisive. The committee takes note of where the median legislator (in the full legislature) stands relative to both the status quo and the committee's ideal point. If the ideal budget is destined to win against the status quo, the committee simply goes ahead and proposes it. If, however, the median legislator prefers the status quo to the committee's ideal point, the committee adjusts by proposing the "closest" budget to its ideal that can win—which may often mean proposing the status quo itself. Thus, regardless of the preferences of the median legislator, the committee is always able to use its agenda powers to guarantee budgetary outcomes somewhere between the status quo and its own ideal point. (For further details concerning the committee, see the Appendix.)

Suppose the committee consists of the five most proconsumer members of the legislature. Agenda control allows such a committee to block any decreases in the budget that may be desired by the full legislature, and, should the full legislature favor increases in the budget, the committee can impose budgets that are still larger than the median legislator would have adopted had his agenda not been controlled. Over time, then, it would seem that a proconsumer committee with agenda-setting powers should be able to engineer much higher levels of regulatory enforcement for consumers.

In fact, things do not work out this way. Version 1-2 introduces this proconsumer committee into a model world that is otherwise identical to the one described by Version 1-1. As Table 1 shows, the committee fails to impose an equilibrium outcome more beneficial to consumers. The system simply moves to the same pluralist equilibrium as before—and again, this result is independent of the starting point.

What happened to the presumed power of agenda control? Why did the committee fail to impose a *structural equilibrium*?[5] The answer is that the committee did indeed have substantial power over outcomes but only in the short run. Like other legislators, the committee members move toward moderation over time in response to constituency forces and, in their oscillatory adjustments, even occupy probusiness territory for a while. In the end, the median committee voter is virtually indistinguishable from the floor median, and the system's equilibrium properties are essentially unaffected by what would otherwise seem to be a major structural change.

The disappearance of what was initially a substantial degree of committee power testifies in dramatic fashion to the importance of recognizing the endogeneity of legislative policy preferences. Were legislators rooted to their initial policy positions, the committee's agenda powers would have been overwhelming and permanent, which is precisely what most models of agenda control lead us to expect. Once we allow legislators to respond over time to constituency influences, as they obviously do in the real world of politics, we gain a very different perspective indeed.

Agenda Control and Committee Capture

Clearly, if agenda control is to work for them, consumers must do something to see to it that the committee does not abdicate its initial proconsumer position. Thus far, the model has been based on an assumption of symmetry: business and consumer groups implicitly follow exactly the same rules in allocating their resources to legislators. In particular, they both contribute for or against a legislator in proportion to the legislator's position along the budgetary scale (that is, according to the size of the budget increase or decrease he or she favors). The way for consumers to capture the committee, it seems, would be to single out committee members for special treatment—to direct more resources their way than their policy positions would otherwise dictate; if they are proconsumer, they should be disproportionately rewarded; if they are probusiness, they should be disproportionately punished.

To understand the consequences of asymmetric group behavior, it is better to begin by temporarily factoring out the element of agenda control. Thus, in Version 1-3 we assume that consumers alter their allocation rule in an effort to capture the committee but that the committee, in fact, has no power to set the legislature's agenda. Under this scenario, consumers are obviously making a mistake by concentrating their resources disproportionately on legislators who have no more power than their colleagues, and we would expect this to affect consumer policy success.

As outlined in Table 1, this is just what happens. On the assumption that consumers set aside 20% of their political resources for expenditure on committee members (over and above what they would normally receive), the system no longer arrives at the former pluralist equilibrium but rather comes to rest at a lower enforcement level of 145. This is not simply a lower-level pluralist equilibrium since consumers are in fact out-contributing business at that point. Nonetheless, it does represent a balance of group forces in a very real sense. Because consumers are literally wasting some of their resources on powerless committee members, it takes more consumer resource units (dollars, if you prefer) than business resource units to achieve the same political impact. Thus, consumer and business resources can only come into balance, in a political sense, when consumers out-contribute business by precisely the "right" amount. When this occurs, the system comes to rest at what we might call a *modified pluralist equilibrium*, which the enforcement level of 145 in this case represents.

One way to look at this is in terms of opportunity costs. When consumers choose to contribute disproportionately to committee members, the opportunity cost is that they have less left over to contribute to all the other legislators, who will therefore be more susceptible to influence by business. The big question is, What do consumers get in return for paying this cost? The answer is that committee members have nothing extra to offer here since they are no more powerful than anyone else. Thus, consumers are forced to accept the consequences of an inefficient allocation rule—a lower, less desirable level of enforcement.

Now suppose, in Version 1-4, that we reendow the committee with agenda-setting powers. Here we have what would appear to be the ideal match: consumers allocate 20% of their resources to committee members who, in turn, have monopoly power over the legislature's agenda. The most likely scenario, it would seem, is that consumers will be successful in the legislature, enforcement will soar, and the modified pluralist equilibrium will be replaced by a structural equilibrium at this higher level.

Some of these expectations are borne out, as we illustrate in Table 1. Consumers do succeed in capturing the committee, which remains resolutely proconsumer in equilibrium. The committee uses its power relentlessly in imposing budgetary outcomes more favorable to consumers than the median legislator desires, and the modified pluralist equilibrium is indeed replaced by a structural equilibrium: a perpetual standoff in which the median legislator prefers to reduce the budget and the committee proposes and achieves the status quo of no change at all.

But there is a fly in the ointment. While consumers achieve a somewhat higher level of enforcement here than they did in the absence of agenda control, moving the system from 145 to 151, this new level is still below the original pluralist equilibrium attainable when consumers ignore the committee altogether. Consumers do in fact receive a bit of political benefit in return for concentrating their contributions on the committee, but it turns out that these benefits of agenda control to consumers are "small" and not worth their price. The price, once again, is the opportunity cost of diverting resources away from legislators who are not on the committee. Though the committee is more proconsumer than it was in Version 1-2, when consumers did not give it special attention, the rest of the legislature is more probusiness than it had been. The net effect of these two opposed shifts is less enforcement than was achieved in the pluralist equilibrium in Version 1-2. Consumers are better off if they do not attempt to capture the committee at all.

These results contradict conventional wisdom on the efficacy of agenda control and the consequences of committee capture. What the usual accounts, whether theoretical or empirical, tend to overlook are the dynamic adjustments that these factors set off among all the other political participants—adjustments that, over time, undercut the initial advantages that agenda control and committee capture bestow. These other participants, noncommittee legislators in particular, have a kind of power that is less obtrusive and less quickly imposed than the committee's but that ultimately asserts itself with real force.[6]

Any model of agenda control contains these opposing powers, and the general issue is, How do their relative strengths vary as basic properties of the system are allowed to change? The results of Version 1-4 are important in their direct challenge to conventional wisdom, but there is no reason to think there may not be other conditions under which agenda control and committee capture do indeed work as well as we have been led to expect. The most prominent candidate is the initial status quo, which, by assumption, is the reversion level for committee agenda control. Given the demonstrated significance of the reversion level in other models of agenda control (see, e.g., Romer and Rosenthal, 1978), there is clearly reason to suspect that our results may change with the initial status quo.

Our suspicions have been wrong before in this regard. As we saw with Version 1-2, a committee with agenda-setting power cannot prevent movement of the system to its "natural" pluralist equilibrium, and this occurs regardless of the starting point. However, this came about because the committee abandoned its original proconsumer position and went to the center. If consumers took steps to capture the committee, as they did in Version 1-4, the committee would presumably remain proconsumer—and therefore be in a position to impose budgetary increases whenever opportunities arise. In Version 1-4, the committee did succeed in raising the initial level of enforcement through the imposition of higher budgets, but the initial level was so low (80) and its opportunities for budgetary largesse so few that the structural equilibrium (151) fell short of the pluralist equilibrium (169). This suggests that if the initial status quo had been above the pluralist equilibrium, a proconsumer committee could "lock in" these high levels of enforcement and perhaps increase them.

We can test this by creating Version 1-5, which is identical to Version 1-4 except that it begins in the first period with a budget of 300 and a corresponding enforcement level of 240. As the figures in Table 1 indicate, our suspicions are correct: the result is a structural equilibrium at an enforcement level of 242, far beyond the pluralist equilibrium and even farther beyond the structural equilibrium of Version 1-4. Here, therefore, committee agenda power is indeed harnessed to serve consumer interests—but only due to the combination of asymmetric group contribution rules and a high reversion level. Both appear to play necessary roles. The contribution asymmetry produces a captured committee, while the high reversion level gives the committee a crucial tool with which to work: it can preserve and protect consumer interests by doing nothing more than blocking floor action to change the status quo. This it can do quite effectively. Clearly, then, the consumer victory amounted to a preservation of the status quo. As is consistent with the neoclassical models, agenda control and committee capture mounted a good defense, but little offense.

Defense, however, sufficed; the committee members had a major impact on the system, imposing a structural equilibrium very different from the original pluralist equilibrium and leaving consumers much better off than they would have been in the absence of committee power. So, while the consequences of agenda control are less dramatic and more highly qualified than some of the literature leads us to expect, it can obviously play a central role in shaping systemic outcomes, and it represents a resource of potentially great value for political participants.

Agenda Control and Group Allocation Rules

A potential resource for consumers is likely to be a potential resource for business as well. If consumers can concentrate their resources on committee members in attempting to capture the committee and its agenda control powers, business can surely attempt to do the same. This brings up the general question of group strategies and their implications for systemic outcomes.

In this article, we do not model interest groups as unitary rational actors with motives and strategies in the usual sense. We think of them as large, decentralized sectors whose political resources are simply increasing functions of the sectoral benefits or costs imposed by bureaucratic enforcement, and whose allocations among legislators are governed by one or more rules that seem empirically plausible. The rationality of decision makers within sectors suggests there is presumably some relationship between what "works" in politics and how resources are allocated: if concentrating resources on committee members works, then this should tend to be reflected in resource decisions and allocations. Yet the cognitive limitations on decision makers and their distance from politics (in national legislative campaigns, for instance, most contributions come from ordinary individuals, not from Washington-based interest organizations) suggest that such relationships are likely to be imperfect. Thus, while the nature and extent of interest group support is clearly crucial to systemic outcomes in our model, we think it would be a mistake to require that sectoral resources are always generated and allocated for maximum political efficiency, that sectors truly have strategies, or that they engage in gamesmanship. At this early stage of theory construction, we prefer to think in terms of reasonable generation functions and allocation rules and to put them to use in exploring the systemic consequences of alternative patterns of interest group support.

To this point, we have concentrated on consumer allocations and have found that consumers can push the system above the pluralist equilibrium when three factors come together: committee agenda control, disproportionate consumer contributions to the committee, and a "high" initial status quo. If any one of these is lacking, the result is either the pluralist equilibrium or a still lower structural equilibrium. Had we concerned ourselves with business instead, the results would have followed identical lines. Business is only able to achieve enforcement levels below the pluralist equilibrium when the committee has agenda control; business funnels additional resources to the committee, and the initial status quo is "low." When any one element is absent, the result is either the pluralist equilibrium or a still higher structural equilibrium. Whether the committee is initially proconsumer or probusiness is of little importance; due to the endogeneity of legislative policy preferences, the committee quickly moves to the capturing group's side.

In this section, we want to explore the impact of group allocation rules further by taking up the basic question of the symmetry or asymmetry of group contributions. We know that when the committee has agenda control, asymmetric allocation rules—one group tries to capture the committee, the other does not—shape systemic outcomes. What happens, however, when both groups try to capture the committee by allocating equal portions (20%) of their resources to committee members? An answer is provided with the help of Version 1-6, which is identical to Version 1-5 (proconsumer committee with agenda control and initial budget of 300) but for the assumption of symmetric group allocation rules oriented toward capture. The result reported in Table 1 is striking: the structural equilibrium of Version 1-5 is shattered, giving way to the pluralist equilibrium. Thus, even though the committee is initially proconsumer and even though the status quo budget (300) and enforcement levels (240) overwhelmingly favor consumers, symmetric group allocation rules bring the system back close to its "natural" equilibrium. Had we started out at a much lower status quo very unfavorable to consumers, moreover, the result would have been the same: the pluralist equilibrium.

Why does this happen? Why, in Version 1-6, does a proconsumer committee fail to use its agenda control to "lock in" the initially high status quo? It fails to do so because at a status quo well beyond the pluralist equilibrium, business is outcontributing consumers in the aggregate. With symmetric allocation rules, this means that all legislators, whether on or off the committee, tend to be more responsive to business than consumer interests. As legislators subsequently shift in a probusiness direction, it does not take long for consumers to lose all of their initial advantages. The committee's agenda control is exercised throughout, but it has no impact on the final result.

Asymmetry is therefore a fundamental determinant of whether agenda control will be of real consequence, for one group must capture the committee if agenda control is to alter the system's equilibrium. However, this only works as long as the other sector does not follow suit. If there is a tendency within the interest group system for relevant information to be disseminated, if groups and individuals tend to imitate one another in their allocations, if common rules of thumb tend to arise regarding contribution strategies, or if any other mechanisms are at work in the world that encourage boundedly rational contributors to behave according to the same basic rules, agenda control may be a fairly unimportant determinant of equilibrium outcomes.

The Spatial Model and District-Specific Resources

Empirically, we know there is an important institutional constraint on interest group support and opposition that tends to guarantee some, and perhaps a very substantial, measure of asymmetry. This constraint derives from the election of legislators from geographic districts.

In a system of electoral districts, political resources are not perfectly fungible. Some, like votes, are necessarily district specific. Many others, including money, are in principle free to flow across district boundaries, though they are also generated and allocated within districts. The degree of nonfungibility becomes important when political interests and resources are unevenly distributed across districts, as they typically are, of course, in American politics: farmers, blacks, blue-collar workers, Catholics, and other groups of political relevance tend to be concentrated in some districts and areas of the country and underrepresented in others. Nonfungibility and concentration combine to guarantee that candidates for office are subjected to differing patterns of support and opposition depending on the types of interests that are concentrated in their districts and on the relative tendencies of these groups to mobilize resources.[7]

Conventional analyses of legislative agenda control assume a political world of electoral districts and thus, implicitly, one in which interest groups are constrained to behave asymmetrically. In addition, they generally take as their paradigmatic case the "high demand" committee—a committee whose members come from districts that, due to concentrated interests and resources, generate pressure for high levels of the policy output controlled by the committee. While these models of agenda control are static, they are supported by a familiar and certainly reasonable story about system dynamics: constituency concentration within districts promotes committee capture through the electoral connection; capture then guarantees that the committee's preferred level of output will depart (substantially) from that of the median legislator, and committee agenda powers are then put to use to generate disproportionate benefits for the concentrated group.

From this perspective, our mobile resources model may appear quite inadequate for investigating agenda control. By ignoring electoral districts and allowing resources to flow freely throughout the system, we undercut a crucial foundation for both constituency capture of the committee and the committee's exercise of agenda powers, and it is hardly surprising that we have been led to emphasize the weaknesses of agenda control. It would seem that an alternative institutional model—one founded on electoral districts—should lead us in just the opposite direction.

Is this in fact the case? To investigate, let's assume each legislator is elected from a single-member district and that all political resources are constrained from crossing district boundaries. As in familiar spatial models of voting (Downs, 1957), what now determines each legislator's electoral security is the difference between the legislator's own policy position and the bliss point of his or her district's median voter. Electoral security is a single-peaked function of this difference, reaching a maximum when the legislator's position equals his or her pivotal voter's ideal point.

Each district's median voter is endowed in period one with a permanent bliss point defined in terms of enforcement. In the analysis to follow, we adopt bliss points raging from 100 for the most probusiness district to 200 for the most proconsumer, with the median district having a bliss point equal to the social optimum of 150.[8]

Because only votes matter, politicians are solely concerned with finding the political center of gravity in their districts—their median voters. Further-

Table 2. Computer Runs of Model 2: District-Specific Resources

	Simulation Version				
	A	B	C	D	E
Parameters					
Committee has agenda authority?	no	yes	yes	yes	yes
Initial budget and enforcement	very low	very low	very high	bit low	bit high
Outcomes					
Enforcement	150	197	197	170	171
Efficiency	.62	.81	.81	.82	.82
Type of equilibrium[a]	FM	CM	CM	SE	SE

[a] FM = floor median
CM = committee median
SE = structural equilibrium

more, because each district's center of gravity is constant, the incentives facing each politician are unchanging: a proconsumer district remains proconsumer, a probusiness district remains probusiness. Consequently, unlike the model of the first part of this article, there are no forces here that would move a proconsumer committee to the center of the policy spectrum. The committee is captured by proconsumer voters in the first period and remains so throughout. Thus, policy differences between the committee and the floor median will persist. As in neoclassical models of agenda control, the essence of this game is continual struggle between floor and committee.

Let's begin by considering the null case in which the committee has no agenda control. We already know that when resources are perfectly mobile, enforcement moves to the pluralist equilibrium regardless of the initial status quo. What happens when resources are constrained by district boundaries? As the figures for Version 2-A in Table 2 illustrate, the system behaves in a thoroughly neoclassical manner: after hunting about, it eventually stabilizes at the enforcement level desired by the median voter in the median district.[9] Further experimentation would easily show that this general result applies regardless of the location of the ideal point and regardless of the initial status quo.[10] In these fundamental respects, Model 2's behavioral dynamics—while anchored in the adaptive adjustments of myopic, imperfectly informed participants—are completely consistent with the basic median voter results of static neoclassical models.

Now suppose the committee is given the authority to control the legislature's agenda under a closed rule. Neoclassical models of perfect information yield straightforward predictions in such cases. With a proconsumer committee whose ideal point is above the status quo, for instance, the committee would (1) impose the status quo if the floor median is less than the status quo, (2) impose its own ideal point if the floor median is somewhere beyond halfway between the status quo and committee median, and (3) impose a compromise position whose distance from the status quo is twice that of the floor median if the floor median is less than halfway between the status quo and the committee median (Krehbiel, n.d.).

Our model is not based on perfect information. Legislators are handicapped by not knowing their districts' ideal points; they must grope their way toward them in trial-and-error fashion based on electoral feedback. Naturally, because legislators do not know the exact location of their own districts' ideal points, they do not know those of their colleagues either. This means that committee members do not know where the floor median would like to wind up, for even the floor median does not know that. We do assume, however, that in every period the committee knows the current budgetary position of the floor median and that budgetary preferences are symmetric.

Despite the uncertain complications that incomplete information and trial-and-error behavior would seem to involve, the model turns out to behave very much in the manner of conventional neoclassical models. In Version 2-B, in which the floor median (150) is closer to the committee median (198) than it is to the initial status quo (80), the system ultimately moves to an equilibrium (197) virtually identical to the committee's ideal. In Version 2-C, the initial status quo is shifted to 240, and the committee once again achieves an equilibrium nearly equal to its ideal. In both cases, this is just what static neoclassical models of complete information would have predicted.

In Versions 2-D and 2-E, the initial status quo is much closer to the floor median, giving the committee far less opportunity for agenda manipulation. In each case, the nature of the result is again what the neoclassical model would predict: a compromise solution representing a standoff between a committee median that is in favor of more regulation and a floor median that is in favor of less. Numerically, these equilibria diverge a bit more from precise neoclassical predictions than did those of 2-B and 2-C, but because the committee's uncertainty about legislative preferences leads to a trial-and-error search that is not precisely on target, some divergence is to be expected. The significance of the results is that the pattern is quite clearly neoclassical.

Mobile and District-Specific Resources

We can combine the two models by allowing for the relevance of both kinds of resources. Each legislator comes from an electoral district characterized by a particular distribution of voter preferences, and, other things being equal, the legislator's electoral security is enhanced the closer his or her own policy positions are to the district median. But now, other things need not be equal, for the legislator's electoral security is also affected by the allocation of mobile resources across all legislators by business and consumer interest groups, and this source of benefits could lead him or her to adopt positions that depart substantially from the district's ideal point.

A legislator's relative responsiveness to district and "national" forces depends on how he or she weights the two in his or her calculus. For purposes of illustration, we present results for versions in which the two sources of utility receive roughly equal weight, in the sense that a unit percentage change in either has the same impact on a legislator's subjective index of electoral security. The model accommodates, of course, any set of weights we might want to explore.

The versions in Table 3, which simply parallel the versions set out in the first part of this article and in Table 1, provide a summary of results for this combined resource model. Because neoclassical models of district-specific resources are the conventional foundation for analyses of agenda control, two points merit emphasis. First, even though the political world is organized by districts, some resources are now mobile, thus reintroducing the influential role of opportunity costs. When consumers set aside some portion of their mobile resources for the purpose of capturing the committee, as in Version 3-4, they do indeed help create a proconsumer committee—but

Table 3. Computer Runs of Model 3: Mobile and District-Specific Resources

	Simulation Version					
	1	2	3	4	5	6
Parameters						
Committee has agenda authority?	no	yes	no	yes	yes	yes
Interest groups allocate extra resources to committee?	neither	neither	consumers	consumers	consumers	consumers and firms
Initial budget and enforcement	low	low	low	low	high	high
Outcomes						
Enforcement	157	163	146	149	218	177
Efficiency	.83	.78	.77	.81	.81	.80
Type of equilibrium[a]	PE/FM	PE/SE	MPE/FM	PE/SE	SE	PE/SE

[a] PE = pluralist equilibrium
MPE = modified pluralist equilibrium
SE = structural equilibrium
FM = floor median

resources allocated to committee members are resources not allocated to other legislators on the floor, who respond by shifting against consumers. The net effect is less enforcement than consumers could have achieved had they not made any special efforts to enhance their control over the committee (Version 3-2).

Second, consumerists' efforts to capture the committee do combine with a high initial status quo to yield enforcement levels beyond those predicted by the neoclassical model (Version 3-5), but symmetrical behavior by business does more than simply destroy the consumer advantage. While the neoclassical model would predict an equilibrium at the committee median, the actual equilibrium under conditions of symmetry is a compromise between the committee median and the lower pluralist equilibrium (Version 3-6). Thus, symmetry does not "cancel out" the consumer advantage and restore the committee median. Instead, it imposes the pluralist equilibrium as a competing point of attraction that pulls the system toward a more probusiness solution unanticipated by the neoclassical model of district-specific resources.

Empirically, of course, we do not know the relative weights of mobile and district-specific resources. Strictly speaking, they doubtless vary across legislators and districts, and historical developments—for example, the rise of political action committees (PACs)—have certainly influenced their values over time. What our model can do is explore the systemic implications of the various possibilities. In this regard, as we could show with further experimentation, it exhibits an important continuity property. When mobile resources are increasingly important in the legislator's calculus, the equilibria are increasingly close to pure pluralist equilibria. When district-specific resources are increasingly important, equilibria approach those of the pure spatial model. In general, there is a tension between the pluralist and neoclassical equilibria whose resolution depends upon the relative salience of the two types of resources.

Conclusion

Our purpose here is to encourage new perspectives in the study of agenda control, committee capture, and institutional politics more generally. Neoclassical approaches have shed important light on these matters and doubtless will continue to do so. Indeed, our own analysis of district-specific resources testifies to the scope and power of neoclassicism, for its simple, static models of complete information are already capable of generating conclusions about systemic equilibria, agenda control, and committee capture virtually identical to those produced by our far more complicated model of adaptive adjustment.

When resources are mobile, however, a static focus on the dyadic core of agenda control—in this case, the relationship between a legislature and a captured, agenda-setting committee—can be misleading. When this relationship is placed within its larger institutional context, when legislators' policy positions are seen as endogenous functions of constituency influences, and when institutional dynamics are allowed to play themselves out over time, we begin to see agenda control and capture in a different light: as part of a system of political behavior in which action provokes counteraction, power begets adaptation, and all political actors are ultimately bound up with one another.

Viewing these issues from a broader institutional perspective suggests several basic conclusions:

(1) Agenda control works quite effectively under some conditions and not at all well under others. Because of the very nature of politics, a committee endowed with formidable powers may find that the system's internal dynamics tend to drain these powers away, to the point where its exercise of agenda control has little or no effect on political outcomes. With a few exceptions (Enelow and Hinich, 1984b; Krehbiel, n.d.; Miller, 1984), the literature tends to overemphasize its political impact.

(2) The wisdom and efficacy of the capture of committees by interest groups are also overemphasized. Groups can indeed capture legislative committees, but their corresponding influence over policy may be short-lived and misleading; over the long haul, institutional dynamics may well promote policy outcomes that are less desirable than those that could have been obtained by ignoring the committee entirely.

(3) For agenda control to work effectively when mobile resources are dominant, it appears that the institutional rules granting a committee agenda-setting powers must be accompanied by two facilitating conditions: asymmetric group behavior that allows one group to capture the committee and an initial status quo sufficiently high or low relative to the pluralist equilibrium.

(4) When district-specific resources are dominant, agenda control seems to operate just as neoclassical models have claimed, even in a world of incomplete information and trial-and-error adaptation.

(5) When both mobile and district-specific resources are important motivators of legislative behavior, institutional dynamics—and, with them, the efficacy of agenda control—reflect an underlying tension between pluralist and neoclassical equilibria.

These conclusions are derived from an adaptive modeling perspective that is broadly anchored in the Simon-March tradition of organizational analysis. While this tradition has so far been put to little use in the formal analysis of agenda control, we believe that its ideas and methods have much to contribute.[11] Whatever orientation is adopted in future work, however, it seems clear that we must ultimately move toward broader models of institutional politics—models that recognize the agenda-setter as but one component of a larger, dynamic whole.

Appendix

Description of the Model

The model has been described in detail elsewhere (Bendor and Moe, 1985); here we sketch out the period-by-period iterations. Readers interested in the finer details may write to the authors for a copy of the computer program.

Every period the legislature gives the bureau a new budget. Each legislator must vote on the budget. Legislators' decisions are determined by how their previous choices affected their electoral security. If they became more proagency last period and their electoral security subsequently increased, then they will take another step in this direction, with the size of the step determined by the size of their utility gain. The same adaptive logic applies to the other permutations.

Once all the legislators have determined their positions, the five committee members vote on a committee bill, with the median of the five being decisive. (We also use a benchmark model without a committee, wherein the budget is determined by the median legislator.) Because the committee has the authority to pose take-it-or-leave-it alternatives to the floor, the committee's budget is paired against the status quo appropriation.

Once the floor is presented with the committee's alternative, the median legislator decides the final outcome. For example, if the committee wants to increase the budget and the median legislator wants to decrease it, the committee's bill is voted down and the agency receives the previous period's appropriation. If the median representative wants a 1% increase and the median committee member wants a 5% boost, then we assume that the committee sophisticatedly proposes an increase just under 2%, which the floor passes.

The committee members behave adaptively, in that their votes are trial-and-error responses to changes in their electoral fortunes. In this sense, they behave just as their colleagues do. However, because committee members are specialists, they not only know more about the substance of an issue than their colleagues do but also have a better grasp of the political lay of the land. Therefore, our committee is assumed to know the "win set" (Shepsle and Weingast, 1981) against the status quo. (This implicitly means knowing the median legislator's ideal point and that he or she has symmetric preferences.) We also assume that the committee acts sophisticatedly within the win set. These assumptions about the committee's knowledge, its sophisticated strategy, and closed rule authority are strong ones. (See Enelow and Hinich [1984a] for a criticism of the complete information assumptions of many agenda models; see Krehbiel [1986] for some experimental evidence on sophistication.) We have tilted the balance of advantages in favor of the committee. We believe, however, that doing so makes the model's central results that much more interesting.

In addition to passing a budget, the legislature conducts rudimentary oversight, as described in the the text.

The legislature's work ends once it sets the new levels of budget and oversight. These new quantities now become input to the agency's utility, telling it whether the environment approves of its past behavior. Its utility function contains four potential arguments: budget, slack, oversight, and policy (an ideal enforcement level). When relevant, budget and slack increase utility; oversight and departure from the agency's ideal point reduce it. Once the agency knows its utility change, it adaptively sets a new efficiency level. Thus, if it increased its efficiency last period and its utility rose, it will increase efficiency again in the current period.

Efficiency is defined as the fraction of the budget spent on enforcement, and enforcement is a linear function of the money spent on it:

$$\textit{enforcement} = c \cdot \textit{efficiency} \cdot \textit{budget}, \quad (1)$$

where c, a constant, denotes the units of enforcement purchased by a dollar of spending.

The new level of enforcement yields new levels of costs for business and benefits for consumers. The business costs imposed by regulation are convex (increasing marginal costs) while benefits to consumers are concave (decreasing marginal benefits). Specifically,

$$\textit{benefits} = k_1 \cdot \textit{enforcement} - k_2 \cdot \textit{enforcement}^2 - \textit{budget} \quad (2)$$

$$\textit{costs} = k_3 \cdot \textit{enforcement}^2, \quad (3)$$

where k_1, k_2, and k_3 are positive constants.

In the mobile resources model, these benefits and costs prompt consumers and firms to make political contributions to legislators. Consumers' total contributions increase linearly in benefits; firms' total contributions increase linearly in costs:

$$\textit{consumers' political resources} = k_4 + k_5 \cdot \textit{benefits} \quad (4)$$

$$\textit{firms' political resources} = k_6 + k_7 \cdot \textit{costs}, \quad (5)$$

where k_4 through k_7 are positive constants. To represent the organizational advantages of firms, we set $k_7 > k_5$.

Once business and consumer groups decide upon resource levels, they expend them for or against legislators in accord with the legislators' positions on the budget. Firms contribute to the campaigns of legislators who vote for budgetary decreases and to opponents of legislators who vote for increases. Consumers typically do the opposite. (They will support legislators voting for smaller budgets if the taxes funding the agency outweigh the program's benefits). In each case, contributions to a legislator's campaign kitty increase in the absolute value of his or her vote.

In the pure district-specific resources model, each legislator's electoral security is a single-peaked function of the difference between his or her vote on the budget and the ideal point of his or her district's median voter. However, because politicians vote on the budget rather than on enforcement directly, the voters must estimate their ideal points defined over budgets. They use the following linear rule of thumb:

$$\textit{budget bliss}_{i,n} = \frac{\textit{enforcement bliss}_i}{\textit{enforcement}_n} \cdot \textit{budget}_n, \quad (6)$$

where subscript i denotes the i^{th} district and n denotes the period.

Whichever resource is active, legislators must adapt to altered patterns of electoral support and opposition, for as they change their votes on the budget, the interest groups and voters adjust their evaluations accordingly. Hence, changes in the legislators' utilities are anchored in these adjusted evaluations of groups and voters. This new feedback prompts the politicians to adapt by choosing a new budgetary position for the coming period, following the decision rule outlined above. This completes the circle and sends the process into another iteration.

Initializations of the Model

In the pure mobile-resources model, each legislator's first vote on the budget equals the status quo ante budget plus a normally distributed random variable of mean zero and standard deviation of four. In the pure fixed-resources model, the legislators' initial positions equal their respective districts' budgetary bliss points plus a normally distributed disturbance of mean zero and standard deviation of five. In the combined resource model, the legislators' initial positions are weighted averages of their positions in the pure models. The weights reflect the relative importance of the two resources. For example, if the mobile resource is negligibly important, then the median legislator's initial position is negligibly different from his or her position in the pure spatial model.

Notes

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1. For example, Shepsle and Weingast (1984) have recently examined the potency of agenda setting when all decision makers, not just the agenda setter, are sophisticated; Denzau and Mackay (1983) and Enelow and Hinich (1984a) have studied how uncertainty about preferences influences agenda-setting behavior.

2. In addition to passing a budget, the legislature engages in rudimentary oversight, which is activated whenever the agency produces less programmatic activity with a bigger budget. The intensity of oversight increases as the legislature becomes more pro-consumer.

3. For terminological convenience, when only mobile resources matter, we refer to firms and consumers as *interest groups;* when only the fixed resource counts, we refer to them as *voters.*

4. Most neoclassical theories assume that group resources are either completely transferable (see Becker, 1983) or completely district specific (see Shepsle, Weingast, and Johnsen, 1981). For an interesting and unusual combination of these two properties, see Denzau and Munger (1986).

5. Readers noting that an equilibrium exists in this unidimensional policy space, even when the committee is inactive, have suggested that we avoid the term *structure-induced equilibrium.* Discussions with Kenneth Shepsle indicate that he invented that term primarily to refer to equilibria created by institutions out of the potential chaos of majority rule cycles.

6. This opportunity cost of committee capture would also appear in models where the decision makers were cognitively perfect, as they typically are in neoclassical theories. Thus, the perverse effect of paying special attention to the committee follows from the structure of the model—that is, from mobile resources and the endogeneity of the legislators' policy positions—rather than from assumptions about individuals. Of course, completely informed and completely rational agents will, in the face of opportunity costs, always make optimal allocations to the committee and floor whereas boundedly rational agents may persist in a suboptimal allocation because they do not understand the system.

7. It is unclear to what extent political resources are in fact nonfungible, for there is little evidence on the matter. However, the growth of well-organized, nationally based political action committees (PACs), which apparently pay close attention to voting records of congressmen (Poole and Romer, 1984), may indicate a trend towards greater fungibility.

8. Because the politicians vote on the budget rather than on enforcement directly, the voters must estimate their ideal points defined over budgets. They do so via a simple linear rule of thumb (see Appendix), by means of which, for example, if a district's median voter wants twice as much enforcement as the agency is currently generating, then he or she wants the legislative to appropriate twice as much money. This rule of thumb embodies relatively little knowledge of how the agency generates enforcement. It does, however, have several desirable qualitative properties. First, if enforcement equals a voter's bliss point, then that voter is perfectly satisfied with the existing budget. Second, if the voter prefers more (less) enforcement than exists, he or she wants a greater (lesser) budget.

9. Because the interest groups in the spatial model cannot allocate extra resources to the committee, this parameter is not in Table 2. To underscore this difference between the two models, the versions in Table 2 are labelled alphabetically rather than numerically.

10. Furthermore, when the median district stays constant but the other districts' bliss points become more dispersed, the equilibrium enforcement is unchanged. However, there is an effect on the individual legislators' equilibrium votes on the budget: as the districts' median voters spread out, so do the legislators' positions. This position-taking effect has no institutional consequences, but does enhance each legislator's electoral security in his or her own

district. All of this is just as neoclassical spatial models would predict.

11. In particular, we suspect that extending adaptive models into the context of a multidimensional policy space would sharpen the differences between behavioral and neoclassical theories, perhaps, in the process, undermining the convergence we observed here under conditions of district-specific resources. Briefly, because searching for alternatives in a multidimensional world is complex, boundedly rational agents would simplify the problem by using heuristics such as satisficing ("do not propose a new policy if the status quo is sufficiently good"). Given these cognitive constraints on the generation of alternatives, a legislature composed of adaptive politicians would probably be more stable than a neoclassical chamber, particularly when both operate under an open rule. It appears promising to investigate such "cognitively induced equilibria" (CIE). (See Riker, 1982, pp. 209-11, for some interesting remarks about a model of politicians' trial and error in a multidimensional space.)

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NEITHER THE PURSE NOR THE SWORD: DYNAMICS OF PUBLIC CONFIDENCE IN THE SUPREME COURT

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Systematic study of changes in support for the U.S. Supreme Court across time has not been undertaken. Armed with a time series of observations from 1966 through 1984, I provide a description of the ebb and flow of public esteem for the Court. Then I outline and test several plausible propositions about the dynamics of support. Statistical analyses compel the conclusion that apart from a relatively constant core of support, increases in judicial activism, inflation, and solicitude for the rights of the accused decreased confidence in the Court; the events surrounding Watergate and increases in presidential popularity and the public salience of the Court brought about increased popular esteem for the high bench. Previous scholars, based on cross-sections of individuals, have emphasized the public's ignorance of and disinterest in the Supreme Court and judicial policy making. The responsiveness of public support for the Court in the aggregate to political events and shifts in the behavior of the justices stands in stark contrast to the conventional image of United States citizenry as singularly out of touch with and unmoved by the Supreme Court.

In the 1960s and early 1970s, at the same time as and perhaps because of the political and social chaos and the apparent decline of faith in basic values and rules, a small but prolific body of political scientists focused attention on public support for a variety of political institutions. Naturally, because of the importance and visibility of the office and the ready availability of data, the president and presidency claimed the bulk of scholarly energies (Kernell, 1978; Mueller, 1973). Nonetheless a few hearty students of the courts (e.g., Dolbeare and Hammond, 1968; Murphy and Tanenhaus, 1968a, 1968b) have attempted to discover the levels, depth, and bases of public support for judicial institutions among publics in the United States (for work on other institutions, see Dennis, 1966, 1975a; Parker and Davidson, 1979; Patterson, Hedlund, and Boynton, 1975).

The lack of any formal connection to the electorate and its rather demonstrable vulnerability before the president and Congress mean that the United States Supreme Court must depend to an extraordinary extent on the confidence, or at least the acquiescence, of the public. Mr. Justice Frankfurter stated the quandary more eloquently: "The Court's authority —possessed of neither the purse nor the sword—ultimately rests on sustained public confidence in its moral sanction" (*Baker v. Carr* [1962]; see also *United States v. Lee* [1882]). Virtually all scholars agree that the Supreme Court plays a crucial role in the making of national policy (Casper, 1976; Dahl, 1957; Shapiro, 1979), but if the Court is the

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"least dangerous branch," how do the justices manage to hold sway much of the time even in the face of substantial "lawmaking majorities" (cf. Barnum, 1985)? To account for this anomaly, some "commentators have attributed the Court's endurance as a national policymaker to its special status with the public" (Adamany and Grossman, 1983, p. 406).

For the most part, lamentably, empirical researchers have painted a dreary and unencouraging portrait of public attitudes toward the Court. Citizens know surprisingly little about the Court and the workings of the judicial branch, manifest scant concern about its personnel and about most decisions, and offer support contingent upon agreement with specific public policies (for reviews, see Adamany, 1973; Daniels, 1973). This brief set of generalizations, understandably enough, stems from data collected in cross-sections of the public. Yet, despite the widespread consensus these days about the importance and desirability of developing an understanding of the dynamics or temporal dimension of public opinion and other political phenomena, no one has undertaken a study of changes in public support for the Supreme Court across time (cf. Handberg, 1984). Fortunately, since 1966 Louis Harris Associates and the National Opinion Research Center have queried members of the American public on a relatively regular basis about confidence in a number of institutions, including the Court. In this article, armed with a time series of observations from 1966 through 1984, I provide a description of the ebb and flow of public esteem for the Court and then develop and test several plausible propositions about the dynamics of support.

I begin by reviewing some of the previous work on public evaluations of the Supreme Court and by briefly introducing the essentials of patterns of confidence in the Court from 1966 through 1984. I then present alternative explanations of confidence in the Court and, finally, statistical results and interpretations. The statistical analyses lead me to conclude that increases in judicial activism, inflation, and solicitude for the rights of the accused decreased confidence in the Court; the events surrounding Watergate and increases in presidential popularity and in the public salience of the Court brought about increased popular esteem for the high bench. The responsiveness of public confidence in the Court to political events and to shifts in the behavior of the justices stands in stark contrast to the conventional image of U.S. citizenry as singularly out of touch with and unmoved by the Supreme Court.

Public Evaluations of the Court

Prior to the advent of modern political science, constitutional commentators made intriguing claims about the public's regard for the Supreme Court. Thus, for example, Lerner (1937) waxed eloquent in a classic essay about a deep-seated, psychological need for security among the public that was requited in the United States by worship of the Constitution and transferred to the justices as interpreters of the basic law (see also Baas, 1979, 1980; Baas and Thomas, 1984; Jackson, 1954, p. 23; Levinson, 1980; Miller, 1965). This image of a revered and deified Supreme Court gathers scant support in the much more critical and human portrayals in the empirical research conducted since the 1950s. To be sure, in a study of the socialization of children, Easton and Dennis (1969, p. 278) lent some empirical credence to the views Lerner (1937) and others had articulated, but based on analysis of children's responses to open-ended questions, Caldeira (1977a, p. 864) found no evidence that children possess any detailed knowledge of the Court and its functions or see the justices as particularly omnipotent, omnicompetent, and beneficent; for the most part, chiren view the Court with indifference.

Research on the attitudes of adults reveals that there is only a relatively shallow reservoir of knowledge about or affect toward the Court in the mass public. Thus, for example, less than half of the public in 1964 and 1966 could name one or more specific decisions of the Court (Murphy and Tanenhaus, 1968a, p. 277; see also Casey, 1976; Dolbeare, 1967; Kessel, 1966). Many apparently clung to a belief in the myth of mechanical jurisprudence in the Court (Casey, 1974). Few members of the public, regardless of the place or time of the sample, fulfill the most minimal prerequisites of the role of a knowledgeable and competent citizen vis-à-vis the Court. Yet on more general evaluations of judicial performance, the public has, for the most part, given the Supreme Court more favorable than unfavorable marks (Dolbeare, 1967; Kessel, 1966; Murphy and Tanenhaus, 1968a). From the analysis of national surveys executed over a period of 30 years (1937–1966), Dolbeare and Hammond (1968) contend that the Court in fact enjoys widespread, though probably shallow, respect among the public (see also Dennis, 1975b; Dolbeare, 1967; Kessel, 1966). On the other hand, scholars report that favorable evaluations of the Court's performance have declined (Dolbeare, 1967; Murphy and Tanenhaus, 1968a; Tanenhaus and Murphy, 1981).

If the public refuses to offer unstinting praise of the Court's performance in general, citizens consistently cast an even more jaundiced eye on specific decisions of the justices. Asked about particular decisions of the Court, respondents to a national survey in 1966 volunteered three times as many negative as positive remarks (Murphy and Tanenhaus, 1968a, p. 287; cf. Adamany and Grossman, 1983, p. 411; Dennis, 1975b, p. 20). Over the 10 years of Murphy and Tanenhaus's panel (1966–1975), the ratio of dislikes to likes declined to a slight extent, indicating perhaps a modest increase in support for specific policies of the Court (Tanenhaus and Murphy, 1981, p. 31). On the visible issues of the day, however one couches the issues, most people find fault with the choices the Court makes.

Scholars have offered a number of explanations for patterns of public support. For example, Murphy, Tanenhaus, and Kastner (1973, p. 50) identify "absence of concern about federal power, youthful idealization, political partisanship, a view of government as a monolith, sympathy for the underdog, liberalism, and agreement with particular decisions" as chief candidates in the search for an understanding of diffuse support. Yet, with all said and done, they conclude that attitudes toward public policy, regardless of the source, provide the best explanation of support. In effect, individuals classified on several items as "liberals" approved of the Court's policies, and the "conservatives" did not. Dolbeare and Hammond (1968), based on earlier studies, purported to show a relationship between partisan identification and association with the president on the one hand and support for the Court on the other. Democrats and adherents of the incumbent chief executive manifested significantly more support than did their counterparts (see Casey, 1976; Dolbeare, 1967, pp. 205–6). Still others suggest a connection between support for state courts and support for executives and legislatures (Lehne and Reynolds, 1978). At various times and places the following have emerged as significant determinants of support: political activism (Adamany and Grossman, 1983); membership in an elite (Beiser, 1972; Caldeira, 1977b; Murphy and Tanenhaus, 1970; Murphy et al., 1973, pp. 45–51; Schmidhauser and Berg, 1972); level of education (Casey, 1974; Murphy and Tanenhaus, 1968a); race (Hirsch and

Donohew, 1968); age (Kessel, 1966; Murphy and Tanenhaus, 1968a; Murphy et al., 1973); and social status (Casey, 1974). Subsequent analyses suggest, however, that beliefs about proper public policy tend to overwhelm demographic influences.

Measuring Support for the Supreme Court

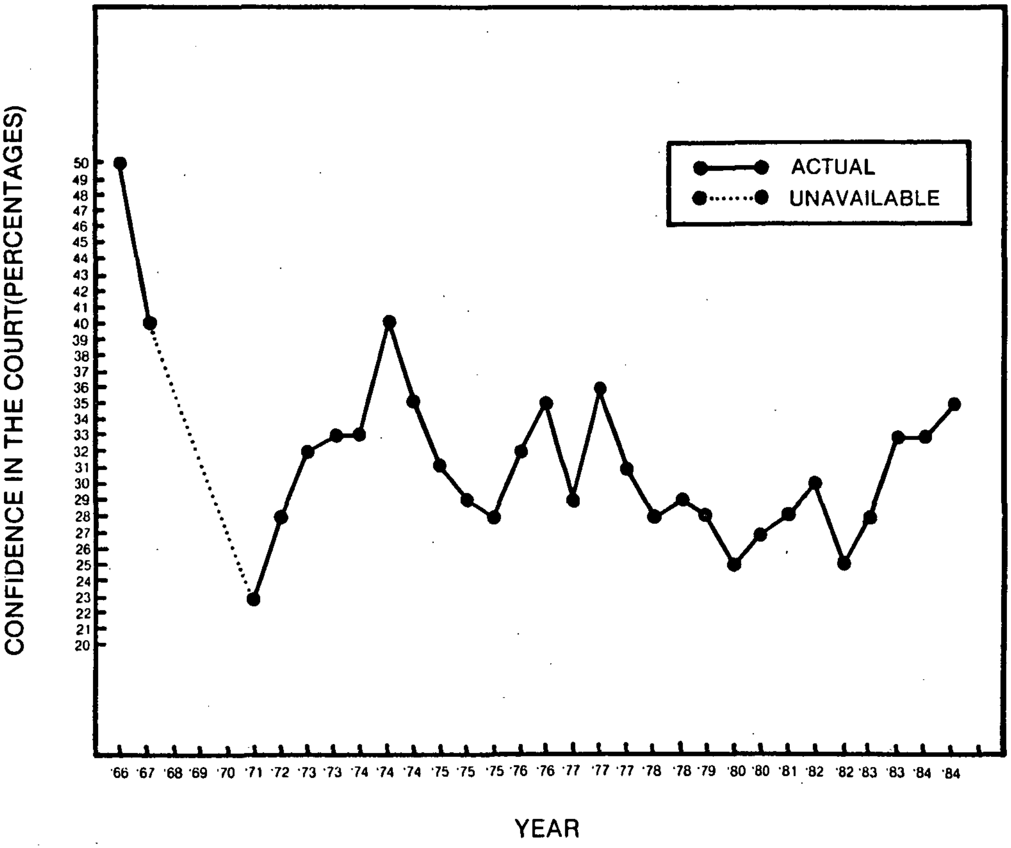
During the storm over Franklin D. Roosevelt's "Court-Packing Plan" in 1937 and in the aftermath of *Brown v. Board of Education*, pollsters invested a fair amount of expense and energy in monitoring public attitudes toward the Supreme Court repeatedly over an extended period of time—in the former case, throughout 1937; in the latter, from 1954 through the early 1960s (Handberg, 1984). Because the interest of elite and mass publics inevitably moved on to new topics, polling operations soon dropped questions on the Court. The wording of items on the Court shifted with the focus of controversy. Thus, even though we now have data on public opinion and the Court from 1935 to the present, we have only recently begun to accumulate a lengthy string of comparable observations. Time-series regression, so often utilized with good effect in studies of presidential popularity, requires a substantial number of observations. Until recently, therefore, scholars of the Court could not study public support systematically across time.

In February of 1966, Louis Harris Associates, for the first time, asked a national sample of Americans, "As far as the people running the Supreme Court are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them?" Nearly a year later, Harris posed the question again and then dropped the matter entirely until August of 1971. In 1973 the National Opinion Research Center (NORC) began to ask the question on an annual basis in the General Social Survey, missing only one year in the last decade. Over the period from 1966 through 1984, NORC and Harris have asked the same question in 29 separate surveys. For students of the Court, these 29 points constitute a rich lode of data. To be sure, one wishes for a larger number; after all, research on public evaluations of the president often includes hundreds of observations. Accordingly, I have taken particular aim at the parsimonious selection of independent variables.[1]

To measure support for the Court, I have opted for the percentage of each sample who responded with "great confidence" in the justices. Figure 1, which displays this number from 1966 through 1984, illustrates the ebb and flow of public support for the Supreme Court. To place public attitudes toward the Court in a broader context, I shall also call attention to materials on Congress and the executive branch (Harris, various years; National Opinion Research Corporation, various years).[2]

Quite clearly, public confidence in the incumbents of the Court has taken a sharp secular decline from 1966 through 1984. More precisely, during this period support has on average dropped 2.2% each year ($Y = 35.5 - 2.2 \cdot Time$; $r^2 = .50$). Whereas about half of the public registered great confidence in the Court early in 1966, only one-third did so during 1984. Even if one considers 1967 as the base, support has dropped by a rather considerable amount. Public confidence in the Court hit rock bottom in 1971 and reached apogees in 1966 and 1974. Of course we have no way of knowing whether the degree of support recorded in 1966 typified public evaluations in the mid-1960s or whether it was entirely out of step. Confidence in the incumbents of the Court, unlike diffuse support (Handberg, 1984; Tanenhaus and Murphy,

Figure 1. Patterns of Confidence in the Court: 1966–1984



1981), has varied a great deal, as the jagged pattern attests. For the entire period, support has a mean of 32%, a standard deviation of 5.5, and a range of 27%. Much of the variation, to be sure, stems from the rapid decline in the late 1960s. If, for example, we restrict our attention to the 1970s and 1980s, confidence in the Court actually ranges from 23% to 40%, for a mean of 30.5 and a standard deviation of 4.0.

Yet confidence does not bob around randomly; change comes gradually. In the late 1960s and the early 1970s, the decline and then rebound of confidence in the Supreme Court marched in step with patterns for other political and social institutions. For the Court, "Watergate" translated into a major temporary gain of public support. Judges and courts, after all, had stood as bastions of the rule of law against the attempts by Nixon and associates to evade justice. Paradoxically, though Congress played a more central role in Watergate than did the Court, the public gave the incumbents of the national legislature much less credit. By 1978, confidence in the Court was again on the decline, perhaps reflecting a general discontent with institutions caused by economic instability. In the last four surveys, support for the Court, along with that for Congress and the president, has increased markedly, consonant with the many reports of an increase in optimism since the midpoint of the first Reagan administration. Finally, across the entire period, the public has bestowed greater confidence in the incumbents of the Court than in those of Congress and usually by quite a large margin.

Explanations of Support for the Court

Thus far I have established that support for the Court varies considerably over time; of this there can be little doubt. Now, the more significant issue is, What, precisely, accounts for these perturbations in public confidence? It is conceivable, on the one hand, that support for the Court responds to changes in societal conditions: as the quality of life declines, so, too, does faith in the justices. On the other hand, perhaps members of the public react to events, politics, and judicial behavior. Large happenings on the political landscape could shape public perceptions of the high bench. The justices themselves, through their own actions, might increase or decrease esteem for the Court, or public feeling about the Court could simply follow sentiment toward other political institutions. These broad conceptions of the wellsprings of public support for the Court encompass each of the more specific explanations I shall develop in this section and test in the next.

Economic Conditions

Work on presidential popularity, support for Congress, and confidence in institutions in general often indicates a strong and positive relationship between economic prosperity and stability and favorable public evaluations. Chief executives who preside over serious inflation and unemployment normally suffer considerable losses of public support, and unstable prices apparently cause more damage (Kernell, 1978; MacKuen, 1983). There is some evidence that the public holds Congress responsible for our economic well-being: as unemployment increases, so does congressional "unpopularity" (Parker, 1977), and Lipset and Schneider (1983) report strong and negative relationships between confidence in the Court and inflation and unemployment. Obviously the Court has no direct impact on economic performance, and no one would argue that it does. Nevertheless, in the bad times of the 1970s, discontent over economic maladies and lost faith in economic organizations spilled over onto noneconomic institutions. High unemployment, which naturally shakes public certitude about the structure of the economy, may well translate into less support for political institutions, including the Court. It is even more likely that inflation should decrease public support for the Court and other institutions because prices affect everyone in some measure. To measure inflation and unemployment, I have chosen the percentage of change in prices and the rate of unemployment in the quarter previous to the observation on public support (U.S. Department of Labor, various years).

Presidential Popularity

Several scholars have contended that people think of the president and the Court in the same vein (Casey, 1975; Dolbeare, 1967; Dolbeare and Hammond, 1968). The president, of course, nominates all members of the Court. Because the president usually has a number of opportunities to nominate new justices, the Court normally constitutes an integral part of the "ruling coalition" (Dahl, 1957; see Scigliano, 1971). From the results of polls, Dolbeare (1967) reports that respondents are more likely to see the Court in a favorable light when a member of their own party occupies the White House and that those who were inclined to defer to presidential decisions were similarly disposed toward the Court. In a later study, Casey (1975) affirmed the "presidential association" and demonstrated that opinions toward the chief executive have a greater impact on attitudes toward the Court among independents than among partisan identifiers. Furthermore, people who trust in the federal government as a whole evince greater support for the Court (Murphy et al., 1973). We therefore have good reason to expect support for the two institutions to move hand-in-hand, even if people do not connect the president and the Court. If support for institutions in general tends to move upward and downward in unison, then presidential popularity and confidence in the Court should show a significant and positive relationship (cf. Tanenhaus and Murphy, 1981). To measure presidential popularity, I have relied on Gallup's data and used the percentage of each sample who express approval of the incumbent chief executive in the quarter prior to the observation on attitudes toward the Court (Gallup, various issues).

Judicial Activism

For the last 30 years or so, commentator after commentator has taken the Supreme Court to task for getting itself too involved in the making of public policy and the lives of the citizens. Critics and even friends of the Court have often warned that over the long haul, "judicial activism" can and does carry severe penalties. These cautions come in several forms. First of all, some scholars have argued that the Court, by refraining from the use of judicial review, can legitimize the policies of other branches of government. If the Court in fact has the power to confer legitimacy, then excessive activity might devalue the judicial coin. Second, others point out that when the Court invalidates a state or federal law, it acts contrary to the preferences of a "law-making majority" (Dahl, 1957). To rebuff popular wishes repeatedly or on controversial measures, however heroic and appropriate, is to run the risk of public disapprobation and perhaps active resistance. Historically, extended periods or particularly aggravating instances of judicial activism have culminated in congressional attempts to curb the powers of the justices; the Court-Packing Plan of 1937 and the Jenner Bill of 1957 spring immediately to mind (Murphy, 1962).

Though assertions of this kind have great currency, few have subjected them to empirical testing. Lehne and Reynolds (1978) show that during a period of great controversy over a significant judicial decision, the salience of the Supreme Court of New Jersey increased, and evaluations became more dependent on people's views of public policy. Yet in the aggregate, judicial activism seems not to have made much difference; through all of the brouhaha, ratings of the Court did not move consistently in one direction.

Few scholars, commentators, or judges agree on the meaning of the concept of judicial activism. In an ideal world, I would create indicators of judicial activism that reflect all of the facets of this complex notion; here I have chosen a simple measure: the number of times the Supreme Court has declared a federal law invalid (for data and further justification, see Caldeira and McCrone, 1982, p. 110).[3]

Salience of the Court

In the normal course of events, the Court does not rank high in public visibility. At certain junctures, however, the Court becomes a more salient object in the public mind. By taking on tough issues and deciding them in a dramatic fashion, the justices themselves play a major role in focusing attention on the Court. Various mass media are even more central in shaping the agenda for public discussion (Grey, 1972; Newland, 1964). In any event, regardless of the precise reasons, public attention to the Court changes substantially over time. Under certain circumstances, heightened salience should lead to greater comprehension of the Court. Presumably, people who learn more about the complexities of judicial decision making will lend the Court greater support than will the less sophisticated. The gist of the argument is that "to know the Court is to respect it, if not to love it" (Kessel, 1966; Murphy et al., 1973, p. 41).

From another perspective, public visibility might actually contribute to negative attitudes toward the justices. Quite apart from stories at the beginning of the term or on new appointments, much of the attention to the Supreme Court in the mass media derives from particular decisions. Clearly, controversial decisions merit much more extensive coverage, usually of a highly divided court. Since the Court often surfaces in the mass media in divisive and politically-charged situations, members of the public might well associate the high bench with trouble. Under public scrutiny, the Court does not usually show off its best judicial face—a harmonious, nonpartisan collection of Solomons. Furthermore, these occasions of intensified interest could focus the citizenry on the reality that judges do make law, all the time—a fact that apparently escapes some people (Casey, 1974).

Cognizant of potential flaws, I have nonetheless employed the number of stories on the Court in the *New York Times* in a year as an indicator of public salience. That newspaper of course reaches only a fraction of the citizenry, but a large proportion of national opinion-leaders read the *Times.* One way or another, the *Times'* coverage of the Court filters down into the towns and villages of America. If anything, this indicator overestimates the salience of the Supreme Court.

Crime

During the era of the Warren Court and even well into the Burger Court, members of the public blamed the Supreme Court for all manner of social maladies—permissiveness, communism, pornography, venereal disease, and the breakdown of the family. An ever-expanding rate of crime figured prominently among the ills ascribed to the Supreme Court. Certainly, an image of the Court as an all-powerful entity unloosing the fetters on social disorder and moral decay does not comport well with the quite circumscribed powers most scholars envision. Even if the Court did have a hand in encouraging the lawless, most academics agree that other influences (e.g., demographics) account for much of the increase in the rate of crime during the 1960s and 1970s. Still, the rate of crime did skyrocket in the midst of the Warren Court's heyday, a time when the accused and the convicted seemed to win a victory every week, and so it is not surprising that some should conclude that the Supreme Court was at fault.

At any rate, when crime goes up, the Court constitutes a logical and handy target for public displeasure. Indeed, of all of the problems that plague the nation, crime provides one of the best tests of the efficacy of the legal and political processes. Courts, after all, have the responsibility of trying and punishing the accused. Just as citizens hold the chief executive responsible for inflation and unemployment, so, too, might the public view the control of crime as peculiarly within the ambit of the Supreme Court. Fault, once placed at the doorstep of the Court, could result in a decline in the public's confidence in the justices. To test this notion, I have used the change in the rate of crime in the quarter previous to the collection of the data on confidence (U.S. Department of Justice, various years). Following the considerations I have outlined, I project that as the rate of crime increases, the level of public support for the Supreme Court will decline.

Judicial Solicitude for the Rights of the Accused

Related to the previous hypothesis, if members of the pubic become concerned about crime and make a connection between it and the Court, we might reasonably expect some movement in public support as the Supreme Court shifts in one direction or the other concerning the rights of the accused. Statements of the justices both on and off the bench suggest a belief among them that judicial protection of criminal defendants normally goes against the tides of public opinion and often translates into public anger. The public need not monitor the subtleties of changes in the criminal law, and everything we know about citizens' knowledge of the law indicates that, for all but a select few, this would be impossible. Instead, the citizenry might sense the general tenor of the Court's attitude toward the rights of the accused and then reduce or increase confidence accordingly. Since only a small segment has direct contact with the *Supreme Court Reports*, the public would receive cues on the justices' current stance at second- and third-hand from such sources as newspapers, television, radio, and opinion-leaders. In light of the lack of public knowledge, confidence in the Court would probably respond only to relatively gross shifts in judicial policies. This means that we need not worry a great deal over the development of sophisticated and detailed indicators of the content of the Supreme Court's policies in the criminal law. Accordingly, I have relied on the percentage of the cases on criminal law decided against the government as a measure of the Court's solicitude toward the accused in any particular term. There is no apparent difference in the effects of shifts in the Court's state and federal decisions on public support, so I have treated the criminal decisions as one category. The foregoing ruminations suggest that as judicial support for the accused increases, public support for the Supreme Court will decrease.

Political Events

In previous work on support for institutions and leaders, scholars have demonstrated the crucial effects of discrete political events and circumstances on the rise and decline of public confidence. For example, Mueller (1973) persuasively argues that crises in foreign affairs result in "rallying-around-the-flag" and a subsequent increase in the popularity of the incumbent chief executive (cf. Parker, 1977). Unfortunately for the purposes of analysis, events normally associated with the Court seldom cause a splash of the dimensions of the Mayaguez incident or the Cuban missile crisis. Particular decisions sometimes do gain a fair amount of attention in the elite media of communications, but few single cases—with the exception of a bombshell such as *Dred Scott*—have sufficient weight to shift public attitudes one way or the other. Even if we could isolate a number of crises or landmark decisions, the polling organizations have not gathered data on support for the Court often enough to permit a precise reading on the influence of salient events.

Quite apart from the momentous and controversial decisions of the last two decades, such as *Roe v. Wade* and cases on capital punishment, two easily identifiable political events rank far above others in potential effects on public support for the Supreme Court. Many have commented on the spectacular consequences—at least in the short run—of the Watergate scandal on the structure of government and people's perceptions of politicians. To be sure, the revelation of the innumerable "horribles" sent President Nixon's rating, as well as public confidence in politicians in general, down to the depths. Perhaps paradoxically, however, public stock in Congress and the Supreme Court rose dramatically. Hearings in the Senate and, later, the House Judiciary Committee's work on articles of impeachment placed the national legislature in an uncustomarily positive light. The courts, from the lowest levels to the highest, consistently opposed the president's schemes to place himself "above the law" and out of the reach of the judicial process. These judicial heroics culminated in *United States v. Nixon,* in which the Supreme Court unanimously refused to allow the president to shield himself from legal scrutiny with a claim of "executive privilege." Presidential misbehavior in Watergate created an opportunity for the Supreme Court and Congress to serve in a visible manner as bulwarks of democracy and of law and order. Therefore I expect that, apart from other influences on confidence, Watergate increased public support for the incumbents of the Supreme Court. To assess the effect of Watergate, I use a dummy variable, scored 1 for August 1974, and 0 otherwise.

The latter part of the administration of Lyndon Johnson marked the beginning of a longer and rather persistent slide downward of public confidence in government and other institutions. Disorders in the streets of large cities, rebellion among students in universities, the morass in Vietnam, President Johnson's pronounced proclivity for hedging the truth—all these and more spelled a precipitous fall from the grace of the relatively trusting and perhaps naive public of the 1950s and mid-1960s. The precise reasons for the increased distrust, however intriguing, need not detain us (see Citrin, 1974; Miller, 1974), for the central point is that the Supreme Court, along with other basic institutions, fell victim to the new dispensation of pervasive cynicism. This we must take into account. Close observers disagree on the exact juncture at which public trust began to dissipate, but we do know from Figure 1 that support for the Supreme Court took the greatest proportionate drop from 1967 to 1971. Hence I hypothesize that a decline occurred after 1967. To construct a properly specified model of support for the Court, I have included a dummy variable for the Johnson administration, scored 1 for surveys executed in 1966 and 1967, and 0 otherwise. Accordingly, I expect to encounter a positive regression coefficient.

Thus far I have adumbrated several alternative explanations of changes in confidence in the Supreme Court: economic conditions, the rate of crime, presidential popularity, judicial activism, support for the rights of the accused, and political events. Formalization in the fashion of a multivariate model yields:

$$Y_t = \alpha_0 - \alpha_1 X_1 - \alpha_2 X_2 + \alpha_3 X_3 - \alpha_4 X_4 - \alpha_5 X_5 - \alpha_6 X_6 - \alpha_7 X_7 + \alpha_8 X_8 + \alpha_9 X_9 + \epsilon_t, \quad (1)$$

where Y_t represents the degree of confidence; α_1, the rate of inflation; α_2, the level of unemployment; α_3, presidential popularity; α_4, the number of federal statutes the Court declares invalid; α_5, the salience of the Court; α_6, the rate of crime; α_7, the percentage of support for the accused among the justices in a particular year; α_8, the effect of Watergate; α_9, the quantum drop in public trust after 1967; and ϵ_t, random errors. Based on the considerations outlined in the previous paragraphs, I believe we should find α_1, α_2, α_4, α_5, α_6, and $\alpha_7 < 0$, and α_3, α_8, and $\alpha_9 > 0$.

Findings

I present in Table 1 the results of the estimating procedure for the comprehensive model sketched in the previous paragraph. These results portray, at the very least, an encouraging initial step toward a persuasive and powerful explanation of confidence in the Supreme Court. Taken as a whole, the equation provides a good, if not excellent, fit with nearly 80% of the variance explained; six of the nine coeffi-

Table 1. Confidence in the Supreme Court

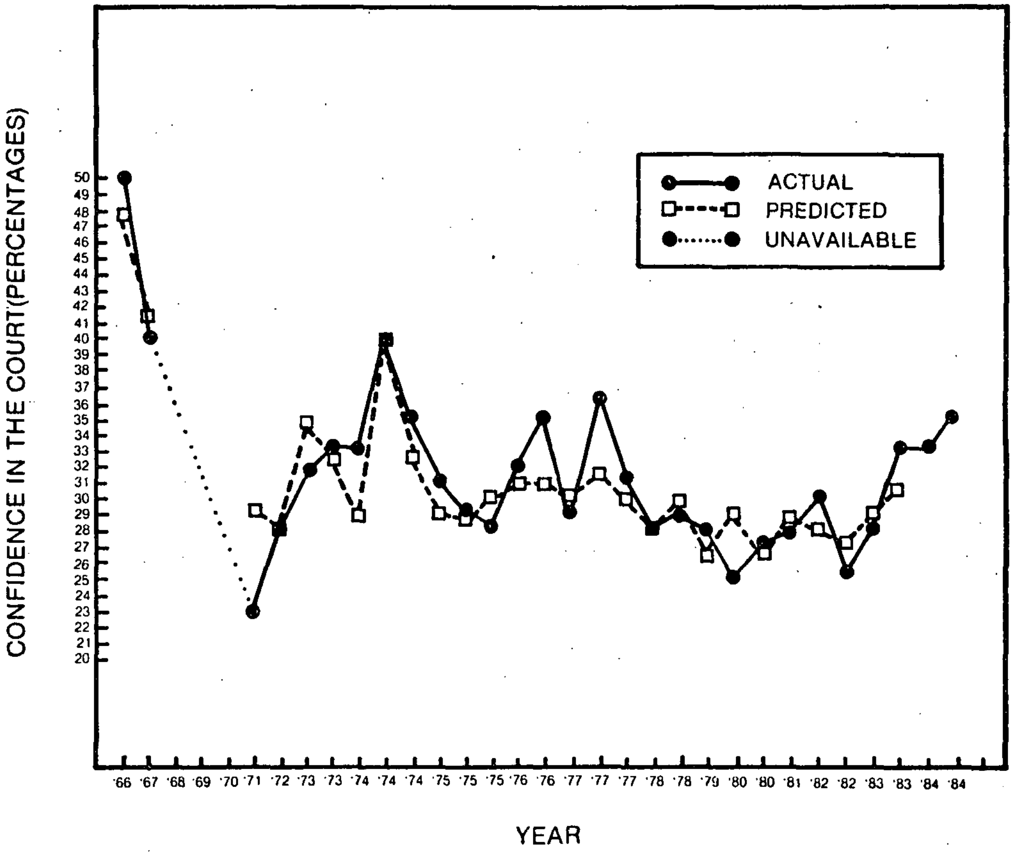
Independent Variables	Estimates[a] b	t ratio	Level of Significance
Intercept	45.44	4.58	.000
Political events/conditions			
Watergate	10.58	2.79	.012
Johnson administration	12.53	2.89	.010
Presidential popularity	0.11	1.85	.082
Social/economic conditions			
Rate of crime	-0.00	-0.01	.986
Rate of unemployment	-0.55	-0.79	.436
Inflation	-0.51	-1.24	.231
Judicial actions			
Salience of the Court	0.08	1.97	.065
Invalidation of federal laws	-2.35	-1.90	.074
Court's support for defendants' rights	-0.21	-1.98	.064
	$R^2 = .79$	$DF = 17$	$N = 27$

[a] After testing for the significance of the autoregressive terms in an initial run, I have excluded them from this equation.

cients reach statistical significance, and the signs of eight of the nine run in the correct direction.[4] Salience in the Court alone goes astray for reasons discussed shortly. According to the intercept, about 45.4% of the public would express considerable confidence in the incumbents of the Court if the several independent variables equalled zero. To be sure, students of presidential popularity achieve uniformly more impressive coefficients of determination, oftentimes as the result of using a lagged dependent variable. Yet the fit in Table 1 compares quite favorably with previous reports on evaluations of Congress (Parker, 1977) and would appear even better had I chosen to run the risk of including a lagged version of confidence in the Supreme Court. Figure 2 illustrates how the estimates track with the actual observations; with the exception of 1971, October 1976, and July 1977, the model performs remarkably well.

Over the period from 1966 through 1984, ebbs and flows of confidence in the Court responded, to a surprisingly large extent, to the happenstances of political events. During and immediately after the consummation of Watergate and the resignation of President Nixon, support for the Supreme Court shot up more than 10 points. The Court benefitted from the president's woes, as I mentioned earlier, in part because of its association with defenses of the "Constitution" against what many in 1973 and 1974 perceived as frontal assaults on the underpinnings of the legal order. Similarly, before wholesale disillusionment and demoralization spread at the end of the Johnson administration, support for the justices ran a full 13% above the norm for the entire period under investigation ($\alpha_9 = 12.5$). Public attitudes toward the Court, as well as toward virtually every element of the American "Establishment," fell in the late 1960s from the lofty pedestals of the years of Eisenhower and Kennedy. The Court may have suffered somewhat more than others from loss of public esteem because

Figure 2. Actual vs. Predicted Patterns of Confidence in the Court: 1966–1984



of perceived connections with problems of race, crime, and cultural disorder, but the justices shared the company of a distinguished group.

Confidence in the Court, unlike support for Congress and the president, reflects the palpitations of economic conditions in what I can most charitably describe as a muted fashion. Bad economic times do not, of course, translate into increases in public esteem for the justices, but they do not seem to hurt a great deal. Thus, for example, the rate of unemployment failed to muster a statistically significant coefficient even as it appeared to manifest the expected negative effect ($\alpha_2 = -0.55$; $p < 0.43$). Inflation presaged, as I hypothesized, deleterious consequences for public support for the Court; for each increase of 1% in the consumer index of prices, the proportion that recorded considerable esteem dropped more than half of 1% ($\alpha_1 = -0.51$; $p < .23$). This relationship, though far from secure, survived a passel of tests I contrived to detect spuriousness. How can we account for the differential effects of inflation and unemployment? For one, although most of us express concern over the plight of the jobless, the rate of unemployment directly affects only a fraction of the citizenry; even at the high tides of economic depression, only a few feel the pinch. To the contrary, because everyone purchases goods and services, inflation, particularly at high levels, presumably holds a greater potential for wreaking havoc with the overall public mood and feelings of confidence. It is commonplace that extended bouts of inflation call into doubt the performance of political and economic actors. That jobs and the stability of prices should on the whole make only a modest dent in confidence in the Court reflects, without doubt, the lack of any direct connection in the public mind between the justices and economic activities. In an odd way, then, the lack of compelling relationships between confidence and economic conditions testifies to the rationality of the ebb and flow of public evaluations of the Supreme Court.

Surprisingly, despite record-shattering rates of change in the 1960s and 1970s, crime as recorded by the F.B.I. had no influence on the amount of confidence the public bestowed upon the Supreme Court. Indeed, of all of the variables, the rate of crime manifests the most diminutive regression coefficient and the least significant *t* ratio in the statistical sense. In the face of steep and very real increases in the rate of crime, the U.S. public apparently failed to hold the Court responsible for this social problem. The middle 1960s, a time of accelerating rates of crime, encompassed high levels of support for the Supreme Court; the late 1970s, a period of markedly less-pronounced movements upward in the incidence of criminal activities, produced relatively low evaluations of the justices among the public. Others have shown that the mass public, in thinking about personal safety, responds not to actual, official reports of the incidence of personal and property crimes but, rather, to the mass media's coverage of the problem of crime (MacKuen, 1979). Thus, if the local newspaper or television station dramatizes the threat of crime, members of the public manifest more anxiety about personal security. Similarly, in making choices on the size of requests and appropriations, the president and Congress apparently weigh the mass media's coverage of crime as heavily as the F.B.I.'s measures of crime (Caldeira and Cowart, 1981; but see Caldeira, 1981). In sum, from the results in Table 1 and earlier research, it seems likely that the media must focus on crime before people express much concern and place blame on the Court.

Proponents of the view that the citizenry associates the justices and the Supreme Court with the presidency can find considerable succor in the statistical results in Table 1. Presidential popularity does indeed exercise a statistically significant, albeit modest, influence on the amount of confidence the public accords the Court. For each 10% increase in popular approval of the president, *ceteris paribus*, public confidence in the Supreme Court increases by approximately 1% ($\alpha_3 = .114$; $p < .082$). Furthermore, in analyses not presented here, the relationship between presidential popularity and confidence in the Court grows ever stronger and more secure in successively more parsimonious versions of the model. These results, moreover, should encourage scholars who have hypothesized that the amount of public support for various institutions tends to move upward and downward together—to a considerable degree in lock step. Members of the public, in this conception, record happiness or displeasure with our institutions across the board. Some auxiliary statistical analyses provide further evidence of this phenomenon. For example, confidence in the Court and in Congress track closely during much of this period.[5] Without more extensive data, however, I cannot make an informed choice between the two formulations outlined here. Let it suffice to say that, for whatever reason, presidential popularity and confidence in the Supreme Court most of the time move in the same direction.

Contrary to conventional wisdom, in the case of the Court, familiarity does not breed contempt—or so the results in Table 1 seem to suggest. Greater public visibility of the Court, in fact, translates into a higher degree of support among the public. For each increase of 12% in the amount of coverage of the Court in the *New York Times, ceteris paribus,* Harris and NORC have recorded on average an increase of 1% in confidence in the Supreme Court (α_5 = .077; $p > .065$). Here, as in some other instances, the relationship gains strength and security in successive variations of the basic model. Some scholars, as I noted earlier, have suggested the possibility that public knowledge of the Court leads to greater support for the justices (Kessel, 1966; Murphy et al., 1973, pp. 41–43). Yet the weight of evidence from cross-sectional studies, I thought, went rather clearly against the happy idea that an appropriately enlightened public would demonstrate a proper appreciation of the Supreme Court (Murphy et al., 1973, pp. 42–43). So the fundamental question remains, Why does public opinion behave in this fashion? It is, after all, difficult to believe that the Supreme Court reaps increased public esteem from publicity on the controversial decisions the justices must inevitably hand down each term. Quite to the contrary, I believe, we can account for the positive association between the salience of Court and public confidence in the justices if we concentrate on the institutional focus of much reporting in the newspapers. Apart from the usual flow of stories on decisions, the *New York Times* and other newspapers on occasion write up individual justices or the Court as a whole, for the most part divorced from the content of the docket. Even so-called investigative stories on the Supreme Court or particular justices almost always present the institution in an extraordinarily positive light. The conjunctions between the ebb and flow of the *Times*'s coverage of the Court and the series on public confidence provide some circumstantial evidence for these speculations. In the final analysis, I suspect the content of the stories on the Court matters a good deal more than the sheer number. That, of course, is an empirical issue and one someone can test with further data.

As expected, judicial activism, in the form of invalidations of federal statutes, cost the Supreme Court dearly in the court of public opinion. Interpretation of the regression coefficient starkly illustrates the high price the Court pays for opposing the will of Congress. For each federal statute the Court has struck down in the past 17 years, public confidence in the justices has declined about 2.4% (α_4 = −2.35; $p < .07$). The losers of litigation, inside and outside the Court, have made such a claim for so long and so many times that the putatively negative consequences of judicial activism have risen to the status of a shibboleth. Nonetheless, the evidence presented here buttresses this ancient pearl of wisdom. To the extent that federal law at any point represents the strong preferences of a majority, the Supreme Court treads on thin ice in the act of invalidating large numbers of statutes. Over the years, as the Supreme Court invalidates federal laws, the justices amass a coalition of enemies, groups and individuals whose views of public policy have lost favor and who will naturally accord the high bench less confidence. Expressed consistently, judicial opposition to Congress may in and of itself lead to resentment and loss of trust among the public—not because of any great love of the national legislature but, rather, as a result of a perception that the Court has upset the balance of our constitutional system. Citizens may indeed view Congress as the "broken branch," but they also have a stake in the process by which national officials check one another. There is, of course, no way of knowing with these data why the public responds in a negative fashion to judicial negations of congressional statutes. Regardless of the precise mechanism, however, I have adduced strong evidence that judicial activism does exact a significant cost in the coin of public confidence.

Public support has, in fact, responded in a significant fashion to changes in the Court's solicitude for the rights of individuals accused of crime. As the Supreme Court has manifested proportionately greater sympathy for criminal defendants, the public has in turn registered a sharp drop in confidence in the justices. More specifically, for every increase of 10% in the share of cases decided in favor of the accused, public support has declined somewhat more than 2% ($\alpha_7 = -0.21$). Two percent does not sound like a devastating blow, but concrete applications provide a graphic illustration of the consequences of the Court's drift in public policy toward the accused: as an empirical matter, judicial support for criminal defendants varied from 12% to 83% during the period from 1966 to 1984, and a shift of 20% or more from year to year happened several times. It goes without saying that members of the public do not make subtle distinctions in their thinking about the direction of the criminal law, and in view of the soup-like quality of individual responses to law and legal institutions, I do not wish to argue that the citizenry forms sharply defined conceptions of the Court or its policies. However, these results suggest that the mass public as a whole responds in a systematic fashion to shifts in the public policies the justices enunciate. Clearly, the public has little sympathy for either the esoterica of criminal procedure or the people who most often utilize these safeguards and apparently translates these attitudes into lack of confidence in the Court. Thus, like judicial activism, solicitude for the accused exacts significant costs to the Supreme Court among the citizenry.

Conclusion

Public support for the Supreme Court varies a great deal over time and eludes simple accounts, but in this article I have attempted to trace meaningful shifts in confidence and, more importantly, to frame and evaluate alternative explanations of the dynamics of esteem for the Court. These rival hypotheses include political events; the rates of crime, unemployment, and inflation; presidential popularity; judicial activism; salience of the Court among the public; and the justices' solicitude for the rights of the accused. Broadly speaking, I conclude that during the period from 1966 through 1984, crime and economic conditions played relatively small roles as determinants of public confidence and that political events and judicial actions registered striking impacts on changes in the public's view of the justices. More specifically, in descending order, the Johnson administration, Watergate, judicial support for the accused, the salience of the Court, invalidations of federal laws, and presidential popularity brought about statistically significant movements in public feelings toward the justices of the Supreme Court during this period.

What do these results mean? Citizens, as individuals, evince little or no knowledge of or concern for the Court; to the extent that they express sensible opinions, they base judgments on the vaguest and crudest of ideological frameworks. Social scientists have demonstrated this over and over again, and yet, in the aggregate, shifts in public confidence in the Court march to the beat of a markedly policy-oriented drummer. Public evaluations of the Court do not float freely, in a seemingly aimless fashion, unconnected to the perturbations of the political and legal processes. Rather, in evaluating the justices, the public appears to respond to events on the political landscape and to actions taken by the Supreme Court. If, for example, the Court adopts a position against a law-making majority, the public accordingly exacts a cost in confidence—a rational calculus indeed. In broad outlines, then, the dynamics of aggregate support for the Court bear a remarkable resemblance to those for Congress and the presidency (Kernell, 1978; Parker, 1977). It is easy to forget that individuals' responses to the presidency and Congress hardly constitute a model of clarity or stability, for scholars of these institutions have spoiled us with crisp, neat political explanations of presidential popularity. Until now, however, scholars of the Court have worked solely with individual-level data and have, naturally enough, emphasized the poor quality of public evaluations of the justices. In this article, with aggregate data, I have portrayed the relationship between the public and the Court as somewhat more rational and calculated than have previous researchers. I do not, of course, wish to press my claim for rationality very far, but I do think that in the literature we have oversold our pessimistic view of public evaluations of the Court, just as students of elections for many years unfairly characterized voters as ignorant and apathetic. For the Supreme Court, as for Congress and the presidency, the movement of public—as opposed to individual—opinion comes much closer to meeting our conventional prescriptions.

Notes

I appreciate the assistance of Brad Lockerbie, a graduate student at the University of Iowa, who gathered much of the data for this project. Chia-Hsing Lu of the Laboratory for Political Research at Iowa helped in the construction of files and the workings of SAS.

1. On a number of occasions during the 1970s, Gallup asked the public, "Would you tell me how much confidence you, yourself, have in each one [a list of institutions, of which the Court was one]—a great deal, quite a lot, some, or very little?" Gallup apparently taps a phenomenon one might label *diffuse support.* Such support in fact and by definition varies little and offers scant play for the investigator. Indeed, one scholar has argued, in part on the basis of Gallup's polls, that public ratings of the Court have changed little in 50 years (Handberg, 1984). By contrast, NORC and Harris focus on the performance of the current justices, very much as in evaluations of the president. A respondent in this context introduces policy-oriented considerations as a matter of course—or at least more so than in responding to Gallup's items. Since I aim to disentangle the connection between public opinion on the one hand and social conditions and political events on the other, the data from Harris and NORC suit my purposes well.

2. In Figure 1, and later in Figure 2, I have chosen to include the years 1968, 1969, and 1970 in order to retain a clear temporal sequence. Instead of interpolating for these years, I have used only actual observations in the statistical analyses.

3. Of course, I could just as easily have used the number of state laws the Supreme Court invalidated in a year as an indicator of judicial activism. I chose federal law instead because I wanted to capture conflict between the Court and national law-making majorities. In future work, I hope to refine this measure to take into account the age of law-making majorities.

4. Because of the ample specification of the model, serial correlation does not pose a threat here. For that matter, in many diagnostic analyses I detected no signs of intolerable levels of multicollinearity.

One reader has raised a question about the effects of the very high observations of confidence in the Court in 1966 and 1967 on the estimates of the model's coefficients. These observations, he suggested, might overwhelm the rest of the series and color all of the results. To test this notion, I removed the data for 1966 and 1967, deleted the variable for the Johnson administration, and ran the statistical analysis again. The new results are quite similar to those presented in Table 1; the main difference is that the quality of the fit declines.

5. Yet in preliminary multivariate analyses, I estimated the effect of confidence in Congress on support for the Court and found no significant relationship. That, of course, indicates the spuriousness of the simple relationship between the levels of confidence in the two institutions.

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FREUD AND FREEDOM OF SPEECH

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In this essay I develop a psychoanalytic defense of freedom of speech that is implicit in Freud's works, principally in his discussions of verbal slips and jokes. Freud argues that freedom of speech benefits people by providing a harmless outlet for aggression, suggesting that it is better to express aggression in words than in violent deeds or to repress it altogether. The psychoanalytic defense of free speech has affinities with various liberal defenses, but it is partial because apolitical; it emphasizes the emotional self-expression of speakers as opposed to the rational persuasion of listeners. The intellectual roots of the contemporary concern with "freedom of expression" (as opposed to "freedom of speech") can be found in Freud: to focus on freedom of expression is to ignore the qualitative differences among forms of self-expression and to neglect the specifically political character of speech.

Sigmund Freud was mostly apolitical, both in his life and in his work;[1] to the extent that he had political convictions, however, he described them as liberal.[2] My purpose here is to suggest that, at least with respect to one significant political question, namely, freedom of speech, Freud's liberalism follows directly from psychoanalytic premises. By focusing primarily on two of Freud's books whose political import has not been sufficiently perceived, *The Psychopathology of Everyday Life* and *Jokes and their Relation to the Unconscious*, I will point to the psychoanalytic grounds underlying Freud's advocacy of freedom of speech.

Freud's main reason for writing these two books was not, of course, to defend freedom of speech; he wanted primarily to heighten his and his readers' psychological knowledge, not to indicate the possible political implications that such heightened knowledge might bring. Nevertheless, a number of Freud's observations and remarks in both books implicitly defend freedom of speech. In this essay I will, accordingly, focus upon the liberal position that Freud adopted but was content to relegate to the intellectual background.

Freedom of speech was a question of importance to Freud for a very good reason: speech itself was and is at the center of psychoanalytic therapy. Freud (1890a, p. 283) claims that "words are the essential tool of mental treatment. . . . [The] science [of psychoanalysis] sets about restoring to words a part at least of their former magical power."

Furthermore, Freud coupled his therapeutic concern with speech with a more general concern that he and others be allowed to speak, to write, and to think freely. A number of Freud's pronouncements seem for this reason to place him squarely in the tradition of liberal defenders of intellectual freedom like Milton, Spinoza, Locke, and Mill. In view of Freud's interests, it is not surprising that many of these pronouncements express his belief in the desirability of speak-

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ing freely about sex. For example, he argues against the "astonishment and horror" that he expects his readers to feel upon learning of the sexual matters he discussed with his patient Dora, who was 18 years old at the time of her treatment:

> There is no cause for indignation. . . . It is possible for a man to talk to girls and women upon sexual matters of every kind without doing them harm . . . so long as . . . he adopts a particular way of doing it. . . . The best way of speaking about such things is to be dry and direct; and that is at the same time the method furthest removed from the prurience with which the same subjects are handled in 'society'. . . . I call bodily organs and processes by their technical names. . . . *J'appelle un chat un chat.* (Freud, 1905e [1901], chap. 1, p. 48)

Similarly, in quoting from the memoirs of Daniel Schreber, where Schreber describes himself as "this person who lets himself be f--d," Freud (1911c [1910], chap 1, p. 20) remarks, "I reproduce this omission from the *Denkwürdigkeiten*, just as I do all the peculiarities of their author's way of writing. I myself should have found no reason for being so shamefaced over a serious matter." Finally, Freud's explanation of the reason for his having given the "love instincts the name of sexual instincts" is relevant here:

> The majority of 'educated' people have regarded this nomenclature as an insult, and have taken their revenge by retorting upon psycho-analysis with the reproach of 'pan-sexualism'. Anyone who considers sex as something mortifying and humiliating to human nature is at liberty to make use of the more genteel expressions 'Eros' and 'erotic'. I might have done so myself from the first and thus have spared myself much opposition. But I did not want to, for I like to avoid concessions to faintheartedness [*Schwachmütigkeit*]. One can never tell where that road may lead one; one gives way first in words, and then little by little in substance. (Freud, 1921c, chap. 4, p. 91)

Freud's defense of intellectual freedom extends beyond discussions of sexuality however. In fact, he criticizes the censorship that he believes people impose upon themselves with respect to thinking about their sexuality in part because he believes the censorship is extended to other matters:

> How can we expect people who are under the dominance of prohibition of thought to attain the psychological ideal, the primacy of intelligence? . . . So long as a person's early years are influenced not only by a sexual inhibition of thought but also by a religious inhibition and by a loyal inhibition derived from this [i.e., a political inhibition deriving from unthinking loyalty to divine right monarchy], we cannot really tell what in fact he is like. (Freud, 1927c, chap. 9, p. 48)

That Freud's defense of intellectual freedom aims at more than the extension of sexual freedom is most clearly established by the following consideration: even though Freud believed in 1938 that "in Soviet Russia they . . . have been wise enough to give [people] a reasonable amount of sexual liberty," he still criticized its leaders for having "robbed them of any possibility of freedom of thought" (1939a [1937-9], prefatory note 1 to essay 3, p. 54).

These statements suggest that Freud defends intellectual freedom for reasons similar to some of those motivating earlier figures such as John Stuart Mill. Freud, like Mill, appears to advocate freedom of speech and thought in order to facilitate the intellectual progress of the individual and the human race—in order to foster personal growth or development toward "the psychological ideal, the primacy of intelligence." Nevertheless, one senses a significant difference in tone between Freud and Mill: underlying Freud's praise of honest speech is an apparent hostility or aggressiveness directed against conventional "society," the "fainthearted" home of "shamefaced[ly]" hypocritical or dishonest speech. Freud evidently prides himself on his honesty, which leads him both to express his anger at society and consequently to anger society. For Freud, intellectual freedom in some way appears to connote not only the search for truth but also the willingness to give vent to one's own aggressiveness and to brave the aggressiveness of others. I believe this is significant, for we will see that aggression, both as manifested by Freud and as understood by him, is central to what is original and psychoanalytic in the Freudian defense of freedom of speech and emotion (which differs in important ways from the traditional and intellectual "Millian" defense of freedom of speech and thought).

In order to understand Freud's specifically psychoanalytic grounds for defending freedom of speech, it is useful to begin with two statements in which he and his one-time collaborator Breuer explain the reason for the importance of speech in psychoanalytic therapy. In the first, they write that "language serves as a substitute [*Surrogat*] for action; by its help, an affect can be 'abreacted' [i.e., a disturbing emotion can be discharged] almost as effectively [as it can by an action]" (Freud and Breuer, 1893a, p. 8). Developing this point in the second statement, Breuer adds that "telling things is a relief" (Freud and Breuer, 1895d, chap. 3, sec. 3, p. 211).

However, these statements are primarily of interest not because of what they tell us about the function of speech within psychoanalytic therapy but because Freud's view of the social function of ordinary speech in some ways resembles his view of the function of the speech directed by a patient to his or her analyst. In society, as in therapy, Freud believes that "language serves as a substitute for action"—that "telling things is a relief." These beliefs lead Freud to construct his psychoanalytic argument suggesting both the advantages and, secondarily, the limitations of freedom of speech. Freud has been described as "the great liberator . . . of speech" (Marcus, 1975, p. 294); by assessing Freud's psychoanalytic argument in comparison with more traditional arguments, my intention is to elucidate this description of Freud and to evaluate its adequacy. I do so hoping to increase not only our understanding of Freud but also our understanding of hte more traditional liberal defense of freedom of speech.

Cursing and Censorship

Freud himself suggests the analogy that I have proposed between the function of speech in therapeutic situations specifically and in social situations generally:[3]

> The most adequate reaction [by which heightened emotion can be lessened] is always a deed. But, as an English writer has wittily remarked, the man who first flung a word of abuse at an enemy instead of a spear was the founder of civilization. Thus the word is the substitute [*Ersatz*] for the deed, and in some circumstances (e.g., in Confession) the only substitute. Accordingly, alongside the adequate reaction there is one that is less adequate. (Freud, 1893h, p. 36; I have made the translation more literal.)

What is crucially important about this statement is Freud's assumption that the first word (or at least the first relevant word in the civilizing process) was a word "of abuse." In saying this, Freud here implies something he states explicitly elsewhere: that social relations are inherently conflictual.[4] One crucial function of speech is, therefore, to provide an outlet for our aggressiveness that is safer, both for us and for others, than physical violence. In an important sense (for which Freud elsewhere provides a theoretical justification) conflict is prior to and more fundamental than cooperation.[5] Because this is already implicit in Freud's statement about the "word of abuse," one can say that Freud's understanding of language in some measure resembles Caliban's in Shakespeare's *Tempest*, for Caliban remarks to Prospero, "You taught me language, and my profit on't / Is, I know how to curse" (act 1, scene 2, lines 363–64).

Language makes it possible for us to curse and profits us by enabling us to work out our hostility to others in a reasonably harmless manner. However, it is to some extent unfortunate, in Freud's view, that we are often unwilling to employ the linguistic vehicle for the exorcism of our aggression because we are unwilling to admit our aggressiveness (and more generally, our egoism) to ourselves. Thus, the aggressiveness displayed by Freud vis-á-vis society and those whom he derisively calls "the masses stems in part from his impatience at their failure to acknowledge and to act out their aggressiveness.[6] Insofar as we are reluctant to acknowledge our aggressiveness, Freud contends that we curtail the freedom of our own speech excessively and harm ourselves by repressing our aggressiveness instead of expressing it in our speech. Free speech should profit us Calibans by enabling us to curse. It does not always do so, however; we suppress our desire to curse because to a great extent—too great—we are Calibans with a bad conscience.

Our bad conscience is apparent in a phenomenon whose very name suggests its relevance to the question of freedom of speech—the phenomenon Freud calls censorship. Freud employs the concept of censorship most prominently in his discussions of dreaming: censorship accounts for the fact that dreams must be interpreted—for the fact that a dream is less "the fulfilment of a wish" than it is "a (disguised) fulfilment of a (suppressed or repressed) wish" (Freud, 1900a, ch. 2, p. 121, and chap. 4, p. 160, respectively).

To explain why dreams are censored would take me well beyond the scope of my argument; for my purposes here, it is sufficient to note what is censored in dreams. Dream censorship is directed against things

> that are invariably of a reprehensible nature, repulsive from the ethical, aesthetic and social point of view—matters of which one does not venture to think at all or thinks only with disgust. These wishes, which are censored and given a distorted expression in dreams, are first and foremost manifestations of an unbridled and ruthless egoism. (Freud, 1916–17, lecture 9, p. 142)

In other words, in our dreams we censor the very things that lead Caliban to enjoy his cursing so much.

To make the metaphorical censorship of dreams comprehensible, Freud employs an analogy, urging us to consider the literal censorship that repressive political regimes impose so as to restrict freedom of thought:

> I will try to seek a social parallel to this internal event in the mind [i.e., dream censorship]. Where can we find a similar distortion of a psychical event in social life? Only where two persons are concerned, one of whom possesses a certain degree of power which the second is obliged to take into account. In such a case the second person will distort his psychical acts or, as we might put it, will dissimulate. The politeness which I practise every day is to a large extent dissimulation of this kind. . . .
>
> . . . A similar difficulty confronts the political writer who has disagreeable truths to tell those in authority. If he presents them undisguised, the authorities will suppress his words—after they have been spoken, if his pronouncement was an oral one, but beforehand, if he had intended to make it in print. A writer must beware of the censorship, and on its account he must soften and distort the expression of his opinion. According to the strength and sensitiveness of the censorship he finds himself compelled either merely to refrain from certain forms of attack, or to speak in allusions in place of direct references, or he must conceal his objectionable pronouncement beneath some apparently innocent disguise. (Freud, 1900a, chap. 4, p. 141)

As we have seen, Freud opposes the literal or political censorship of speech. It is noteworthy, however, that Freud's personal strategy in opposition to censorship differs somewhat from the strategy he ascribes to "the political writer": the political writer "soften[s] and distort[s] the expression of his opinion, . . . speak[s] in allusions in place of direct references, or . . . conceal[s] his objectionable pronouncement beneath some apparently innocent disguise." By contrast, Freud himself prefers to be direct, rather than seemingly accommodating in the manner of the political writer: he calls a cat a cat, "avoid[s] concessions to faintheartedness," and refuses to "give way [even] in words." One is tempted to say that the political writer must mask his aggression whereas Freud prefers freely to proclaim his. This difference between Freud and the political writer is important because it points to an implicit Freudian critique of politics altogether, as a realm of life in which aggression must be hypocritically papered over. We saw at the outset that Freud describes himself as being both liberal and apolitical; it is noteworthy that both of these characteristics apply to Freud's defense of freedom of speech. Freud's liberalism is apparent in his stance as a defender of freedom of speech, but the kind of speech that Freud is most concerned with defending and the grounds on which he defends it also testify to his apolitical tendencies. Freud's psychoanalytic defense of freedom of speech, I will argue, primarily suits speech that is fundamentally extrapolitical, perhaps even antipolitical.

Freudian Slips and Freedom of Speech

To this point we have seen that Freud advocates freedom of speech in part because he views it as a relatively safe outlet for our aggressiveness and egoism. On the basis of Freud's discussion of dream censorship, I have hypothesized that he views our reluctance to admit to our aggressiveness and egoism as a cause of restrictions upon freedom of speech. In order to test this hypothesis and to assess its relevance to politics, I now turn to analyses of *The Psychopathology of Everyday Life and Jokes and their Relation to the Unconscious.* I focus on these works in particular because they are preeminently concerned with language as it is employed by ordinary people in "everyday" social life: ordinary healthy people tell jokes and commit slips of the tongue. Because these books deal with everyday life, a number of Freud's examples in them are directly relevant to the question of freeing speech from the constraints that are often, and perhaps ordinarily, imposed by political and social life.

Freud contends that some slips of the tongue result from our conscious attempts to suppress thoughts that occur to us; these attempts, in turn, can reflect our judgments that certain of our thoughts are unacceptable within our society (or at any rate to certain members of our society) even though the thoughts in question might be fully acceptable in other, conceivably better, societies. This is why Freud's discussion of slips of the tongue is relevant to freedom of political and social speech: Freud contends that some slips occur as a reaction against official governmental censorship of speech and that others occur as a reaction against the more informal censorship imposed by social conventions. Slips of the tongue can break through the censorship that political or social conditions lead us consciously to impose upon ourselves and can sometimes be viewed as a protest against the political or social conditions that, we believe, necessitate the censorship. In this sense, some slips of the tongue can be said to be relative to the regime or society in which they occur. Some of our slips may lead us indirectly to express the truths that we might not hesitate to proclaim openly under governments and in societies where freedom of speech was more widely permitted.

Freud gives a politically relevant example of such a slip. Quoting his pupil Victor Tausk, he tells of Herr A., a Jew who converts to Christianity so as to be able to marry his fiancée. The marriage produces two sons. Some years later, while on a summer holiday, he discovers that his hostess, who is unaware of his religious heritage, is an anti-Semite. Fearing that his sons, "in their candid and ingenuous way, would betray the momentous truth if they heard any more of the conversation, [he] tried to get them to leave the company by sending them into the garden."[7] But he then betrays his heritage himself (although his hosts, being providentially unaware of Freud's work, fail to interpret properly his betrayal): "I said: 'Go into the garden, *Juden* [Jews],' quickly correcting it to *'Jungen* [youngsters].' In this way I enabled the 'courage of my convictions' to be expressed in a parapraxis" (Freud, 1901b, chap. 5, pp. 92–93).

It is reasonably clear why Herr A. misspeaks himself in this example: he knows that by birth he is Jewish and knows that he does not want his hostess to know it. His belief that he must conceal his Jewish birth merely testifies to the fact that he lives in a society in which many people are anti-Semitic: his society confronts him with the unattractive alternatives of "betray[ing]" his Jewish birth either by revealing it against his will or by falsely denying it. In a freer society, in which his Jewish birth would not be an issue, he could admit it, even proclaim it, without being punished for it. Herr A.'s reference to the "courage of his convictions" suggests that his slip embodies a protest against a society in which he believes he will be penalized if he freely speaks a truth about himself.

In a number of other cases, in fact, Freud explicitly states that slips of the tongue can be viewed in this way, as reactions against and expressions of resentment toward political censorship, that is, political suppression of free speech. Thus Freud quotes the example of Professor M. N., a Swiss professor during World War I who was sympathetic to the Allies but who was prevented by Swiss neutrality from referring to the Germans as *boches.* In the course of a lecture, the Swiss professor tells a story to illustrate a point:

> It concerned a German schoolmaster who had put his pupils to work in the garden, and in order to encourage them to work with greater intensity invited them to imagine that with every clod of earth that they broke up they were breaking a French skull. Every time the word for "German" came up in the course of his story N. of course said *"allemand"* quite correctly and not *"boche"*. But when he came to the point of the story he gave the schoolmaster's words in the following form: *Imaginez-vous qu'en chaque moche vous écrasez le crâne d'un Français.* That is to say, instead of *motte* [the French word for *clod*] —*moche!* (Freud, 1901b, chap. 5, pp. 72–73)

Freud quotes the analysis of this example (which he describes as an "exceedingly fine" one) given him by his source for the story, Dr. L. Czeszer:

> Anxiety about committing a political indiscretion, perhaps a suppressed desire to employ the usual word in spite of everything—the word that everyone expected—and the resentment of one who was born a republican and a democrat at every restriction on the free expression of opinion—all these interfered with his main intention of giving a punctilious rendering of the illustration. The interfering trend was known to the speaker and he had, as we cannot but suppose, thought of it directly before he made the slip of the tongue. (Freud, 1901b, chap. 5, pp. 72–73)

It is true that the discussion of republican resentment "at every restriction of the free expression of opinion" comes to Freud from his informant; the words are not Freud's own. Shortly afterward, however, Freud himself makes a similar point in defense of "the free expression of opinion." He does so in discussing an example in which a subordinate's slip of the tongue while proposing a toast to his superior results in his effectively mocking his superior. Freud's analysis of the slip leads him to this conclusion:

> If I were the Principal who was being honoured at the ceremony to which the speaker contributed the slip, I should probably reflect on the cleverness of the Romans in permitting the soldiers of a general who was enjoying a Triumph openly to express in the form of satirical songs their inner criticisms of the man who was being honoured. (Freud, 1901b, chap. 5, pp. 82–83)

The cleverness of the Romans was manifested in their permitting inferiors openly to express their inner criticisms of their superiors. For all that Freud is a critic of the "masses," for all that he is not himself a "democrat," one can understand him to be making a more general claim in this passage: he implies that societies in which the "masses" or the subjects can openly criticize their superiors (at least at times, if not necessarily always) are superior to societies that deny them this privilege.

I will explain why this is so below. In the meantime, however, we should consider a third example, in which Freud discusses a misprint found in an Austrian telegram (sent shortly before the end of World War I) that referred to the "active and *interrupted* co-operation" between Germany and Austria (Freud, 1901b, chap. 6, p. 121, translator's emphasis). Freud comments: "Only a few weeks later it was possible to express one's opinion more frankly about this 'mutual confidence,' and there was no longer any need to take refuge in a slip of the pen (or misprint)" (Freud, chap. 6, p. 121).[8] Just as the word can substitute for the more desirable deed on occasions when it is impossible to act, so can the misprint be a substitute for the more desirable free expression of one's actual opinion on occasions when this is impossible or impolitic.

One thing unites all of the slips that we have examined: they all testify to the aggressiveness of their perpetrators. Each slip reveals an aggressiveness that social considerations often impel people to conceal. Herr A. alludes to his Jewish birth, which would offend his hostess; Professor N. attacks the Germans; the subordinate's toast mocks his superior; an Austrian official expresses his frustration at the terms of cooperation with Austria's German ally.

These examples seem to suggest not only that we wish to speak freely but also what we wish to speak freely: we apparently wish to be freer that we often are to proclaim the truth of our self-centeredness, notwithstanding that the self-centeredness of each individual runs the risk of bringing about conflict among all individuals. I have already pointed out that Freud's emphasis on the conflictual character of human beings is familiar from his later works, yet it is already in evidence in *Psychopathology*. In a revealing passage added to the work in 1907, Freud makes the following observation about himself, and then extends it to others:

> It is as if I were obliged to compare everything I hear about other people with myself; as if my personal complexes were put on the alert whenever another person is brought to my notice. This cannot possibly be an individual peculiarity of my own: it must rather contain an indication of the way in which we understand "something other than ourself" in general. I have reasons for supposing that other people are in this respect very similar to me. (Freud, 1901b, chap. 6, pp. 24–25)

Yet people not only compare themselves with others; they also do not want to be subordinated to others. Freud's most general explanation of the causes of parapraxes provides particularly relevant evidence of his emphasis upon human self-assertiveness:

> In healthy people, egoistic, jealous and hostile feelings and impulsions, on which the pressure of moral education weighs heavily, make frequent use of the pathway provided by parapraxes in order to find some expression for their strength, which undeniably exists but is not recognized by higher mental agencies. (Freud, 1901b, chap. 12, p. 276)

In Freud's view, then, parapraxes testify not only to our desire to speak freely but also to our egoistic character. Insofar as we are egoists, only with difficulty can we adjust to the constraints imposed upon us by social life. Thus, parapraxes suggest there is a conflict between the egoism that characterizes us as individuals and the consideration for others that social and political life demand of us. Twice in *Psychopathology* Freud explicitly discusses this conflict. In one passage, he criticizes conventional politeness: "Since I have come to recognize that I have often mistaken other people's ostensible sympathy for their real feelings, I have been in revolt against these conventional expressions of sympathy, though on the other hand I recognize their social usefulness" (Freud, 1901b, chap. 7, p. 155). In a second passage, he argues that people should express their dislikes openly, rather than allow their parapraxes to express their dislikes for them indirectly: "And this is no doubt the punishment [i.e., that people take offense at our ostensibly unintentional neglect of them] for people's internal dishonesty in only giving expression under the pretext of forgetting, bungling and doing things unintentionally to impulses that would be better admitted to themselves and to others if they can no longer be controlled" (Freud, 1901b, chap. 9, p. 211).

To say that Freud postulates a conflict between individual self-interest and social cohesion is not, of course, to produce an original insight. Freud's major reason for writing *Civilization and its Discontents* was to explain the sources of this conflict, which he thought was chiefly responsible for the discontents that, he believed, characterize civilization. Nevertheless, Freud's argument in *Psychopathology* appears to differ from the later argument in *Civilization* in one important respect. *Civilization and its Discontents* attempts to show why all social orders are deficient but is not particularly concerned with differentiating among social orders; by contrast, *Psychopathology* might seem to suggest that some social orders—that is, liberal social orders, those which provide greater outlets for individual self-interest and for the desire for free self-expression—would therefore be superior to others. Even in *Psychopathology*, Freud implies that all social orders constrain the individuals who compose the; yet if all social orders do not exercise the same degree of constraint and if the degrees to which they do exercise constraint differ significantly, there might be good Freudian reasons for preferring liberal social orders to nonliberal ones.

It is true that Freud does not give us much in the way of normative guidance in *Psychopathology*. He does not tell us how to strike the balance between the "revolt" against "conventional expressions of sympathy" and the recognition of "social usefulness"; nor does he tell us when we should "admit" our hostile impulses to ourselves and when (and how) we should "control" them. Nevertheless, the evidence in *Psychopathology* indicates Freud's support for a society in which people would feel freer to speak and act honestly and openly on behalf of their own self-interest, even at the risk that this would lead to more conflict within society. Elsewhere, Freud provides a good explanation of his position in an argument he makes concerning the behavior of neurotics (which, I suggest, he would apply to social behavior generally):

> A certain number of people, faced in their lives by conflicts which they have found too difficult to solve, have taken flight into neurosis and in this way won an unmistakable, although in the long run too costly, gain from illness. What will these people have to do if their flight into illness is barred by the indiscreet revelations of psycho-analysis? They will have to be *honest*, confess to the instincts that are at work in them, face the conflict, *fight for what they want*, or go without it; and the *tolerance* of society, which is bound to ensue as a result of psycho-analytic *enlightenment*, will help them in their task. (Freud, 1910d, pp. 149–50, emphasis added)

In other words, Freud advocates a liberal society, whose members will accept the extent to which they are Calibans and cease to have bad consciences for it. To achieve this end, that society must be enlightened by psychoanalysis and brought to a state of tolerance—tolerance of the fact that if human beings are honest with themselves and with one another, they will recognize that they must fight with one another for what they want. Free speech is essential for this society; it enables society's members to declare exactly why and for what they are fighting. Furthermore, it has the added advantage of enabling them to fight with one another without physically harming one another, by providing them with the less dangerous verbal equivalent of war.

Jokes and their Relation to Freedom of Speech

Like *The Psychopathology of Everyday Life,* Freud's work *Jokes and their Relation to the Unconscious* emphasizes both the inevitability of conflict and the utility of speech as an outlet for conflict. Freud explains the purposes of jokes in this way: "There are only two . . . and these . . . can themselves be subsumed under a single heading. It is either a *hostile* joke (serving the purpose of aggressiveness, satire or defence) or an *obscene* joke (serving the purpose of exposure)." Jokes "make possible the satisfaction of an instinct (whether lustful or hostile) in the face of an obstacle that stands in its way" (Freud, 1905c, chap. 3, pp. 97 and 101, respectively).

Freud's discussion of hostile instincts is of particular importance for my argument, for it is another clear prefiguration of Freud's treatment of aggression in *Civilization and Its Discontents* some 25 years later:

> Since our individual childhood, and similarly, since the childhood of human civilization, hostile impulses against our fellow men have been subject to the same restrictions, the same progressive repression, as our sexual urges. We have not yet got so far as to be able to love our enemies or to offer our left cheek after being struck on the right. Furthermore, all moral rules for the restriction of active hatred give the clearest evidence to this day that they were originally framed for a small society of fellow clansmen. In so far as we are all able to feel that we are members of one people, we allow ourselves to disregard most of these restrictions in relation to a foreign people. Nevertheless, within our own circle we have made some advances in the control of hostile impulses. (Freud, 1905c, chap. 3, p. 102)[9]

These "advances" consist in the substitution of hostile words for hostile deeds and then in the substitution of ostensibly joking hostile words for serious ones:

> As Lichtenberg puts it in drastic terms: "Where we now say 'Excuse me!' we used to give a box on the ears." Brutal hostility, forbidden by law, has been replaced by verbal invective. . . . Since we have been obliged to renounce the expression of hostility by deeds . . . we have developed a new technique of invective. . . .
>
> . . . We are now prepared to realize the part played by jokes in hostile aggressiveness. A joke will allow us to exploit something ridiculous in our enemy which we could not, on account of obstacles in the way, bring forward openly or consciously; once again, then, the joke *will evade restrictions and open sources of pleasure that have become inaccessible.* (Freud, 1905c, chap. 3, pp. 102-3)

In short, Freud claims that jokes psychologically benefit us by enabling us to express hostility that in the absence of jokes we would feel obliged to suppress.

The political implications of Freud's contention that jokes serve us as a vehicle for the expression of hostility are apparent from Freud's own examples. To begin with, it is of some interest that the contemporary figure whose jokes Freud cites most often is himself a political figure, whom Freud refers to by the alias "Herr N."[10] Herr N. is

> one of the leading men in Austria, who, after important scientific and public work, now fills one of the highest offices in the State. I have ventured to make use of the jokes which are ascribed to him, and all of which in fact bear the same impress, as material for these researches, above all because it would have been hard to find any better. (Freud, 1905c, chap. 2, p. 22)

Without exception, the jokes of Herr N. that Freud quotes are all insult jokes, or "pieces of invective" (Freud, 1905c, chap. 3, p. 103). Herr N.'s verbal aggression must be expressed in the form of jokes, Freud explains, because

> the high position he occupies makes it impossible for him to give out his [critical] judgements [as bluntly as he would like by] . . . exclaim[ing them] aloud. . . . They therefore bring in a joke to their help, and this guarantees them a reception with the hearer which they would never have found in a non-joking form, in spite of the

> truth they might contain. (Freud, 1905c, chap. 3, p. 103)

Herr N. is characterized by

> a strong inclination to invective [that] is held in check by a highly developed aesthetic culture. By the help of a joke. . . . [his] inhibition is lifted. . . . The satisfaction of [his] purpose is made possible and its suppression, together with the "psychical damming-up" that this would involve, is avoided. (Freud, 1905c, chap. 4, p. 118)

Herr N. is a public figure who has "a strong inclination to invective," but his "high position . . . makes it impossible for him" to give vent to his invective freely, "in spite of the truth" its expression "might contain." The example of Herr N. is significant for my argument because he is emblematic of public life as Freud generally conceives of it, both in his desire to insult and in his inability to insult openly. According to Freud, political life gives us cause to want to take out our aggressiveness upon others but also often prevents us from doing so, in words as well as in deeds; in particular, political life causes subjects to resent their sovereigns but often makes it difficult for them to express this resentment.

These characteristics of political life are revealed in the two most politically significant jokes Freud recounts. Both jokes are told of and to sovereigns by their subjects, and both enable the subjects to circumvent restrictions on their freedom of speech and indirectly to make criticisms of the sovereigns that they would be unable to make directly. The first joke concerns the relation of a physician to his king:

> The King condescended to visit a surgical clinic and came on the professor as he was carrying out the amputation of a leg. He accompanied all its stages with loud expressions of his royal satisfaction: "Bravo! bravo! my dear Professor." When the operation was finished, the professor approached him and asked him with a deep bow: "Is it your Majesty's command that I should remove the other leg too?" (Freud, 1905c, chap. 2, pp. 72–73)

The political import of this joke is evident: the physician resents what he takes to be the king's assumption of his subservience to the king. The physician does not feel that he is at liberty directly to express his resentment, however; instead he expresses his resentment indirectly, by making the joke, and thereby "making himself understood by saying the opposite of what he thinks but must keep to himself" (Freud, 1905c, chap. 2, p. 73). The joke enables him, a subject, to criticize his sovereign, notwithstanding his political subordination to his sovereign; it enables him to circumvent the censorship that can restrict subjects' freedom of speech with respect to their sovereign. In this sense, his joke is a blow struck on behalf of equality between subjects and sovereigns, on behalf of the extension of freedom of speech to subjects as well as to sovereigns.

This joke is of particular interest in that one can discern a subjective, autobiographical reason for Freud's recounting of it. Freud himself was a physician who believed that he had for a long time been unjustly subordinated by a monarchy: he believed that political interference from Austria-Hungary's imperial government had been responsible for his having been passed over initially for promotion to a professorship at the University of Vienna[11](and subsequently for his receiving it).[12] One of Freud's teachers spoke to him about "personal influences which appeared to be at work against [Freud] with his Excellency [i.e., the imperial Minister of Education, who was empowered to make decisions concerning promotions at the state-run university] and . . . advised [Freud] to seek a personal counter-influence" (Freud, 1954, p. 343, letter to Wilhelm Fliess of 11 March 1902). When at last his promotion was approved, Freud wrote that it was "as if the role of sexuality had been personally recognized by His Majesty, the interpretation of dreams confirmed by the Council of Ministers, and the necessity of the psycho-analytic therapy of hysteria carried by a two-thirds majority in Parliament" (Freud, 1954, p. 344, letter to Fliess of 11 March 1902). Common to both the physician in the joke and to Freud in real life is a resentment at the political assumption that the work of scientists should be thought to be subject to the judgments, whether approving or disapproving, of politicians.

Nor need we deduce Freud's resentment of subjection to monarchic authority and of the need to placate it from this joke alone. He makes his rejection of kings' right to rule commoners perfectly explicit elsewhere in *Jokes*:

> "We, by the *un*grace of God, day-labourers, serfs, negroes, villeins . . ." is how Lichtenberg begins a manifesto (which he carries no further) made by these unfortunates—who certainly have more right to this title than kings and princes have to its unmodified form. (Freud, 1905c, chap. 2, p. 77)[13]

In addition, Freud's other antimonarchist joke is relevant here:

> Serenissimus was making a tour through his provinces and noticed a man in the crowd who bore a striking resemblance to his own exalted person. He beckoned to him and asked: "Was your mother at one time in service in the Palace?"—"No, your Highness," was the reply, "but my father was." (Freud 1905c, chap. 2, pp. 68–69)[14]

Freud's subsequent analysis of this joke is of great importance:

> The person to whom the question was put would no doubt have liked to knock down the impertinent individual who dared by such an allusion to cast a slur on his beloved mother's memory. But the impertinent individual was Serenissimus, whom one may not knock down or even insult unless one is prepared to purchase that revenge at the price ofone's whole existence. The insult must therefore, it would seem, be swallowed in silence. But fortunately a joke shows the way in which the insult may be safely avenged. (Freud, 1905c, chap. 3, p. 104)

With speech as his vehicle, Serenissimus's interlocutor finds an outlet for the resentment that he feels as a result of his subjection to an insulting and "impertinent" sovereign. Only that he can convert his criticism of the sovereign into a joke enables him to air his criticism; he finds it possible to voice in jest an attack that he could not possibly voice in earnest. In this example, as in the previous one, a joke allows a subject to satisfy the serious desire to criticize his political superior. In this sense, one can regard political jokes as opening wedges in the effort to expand political freedom of speech or, at any rate, as compensating substitutes given prevailing restrictions on freedom of speech.

In his gloss on the subject's retort to Serenissimus, Freud again makes his political point quite clearly:

> Tendentious jokes are especially favoured in order to make aggressiveness of criticism possible against persons in exalted positions who claim to exercise authority. The joke then represents a rebellion against that authority, a liberation from its pressure. The charm of caricatures lies in this same factor: we laugh at them even if they are unsuccessful simply because we count rebellion against authority as a merit. (Freud, 1905c, chap. 3, p. 105)

Thus, like political criticism generally, the joke is a vehicle specifically for the expression of discontent with the inegalitarian relationship between ruler and ruled. In this respect, the desire for freedom to criticize political superiors reflects an egalitarian discontent with the fact that there are political superiors. Freedom of political speech thus acts as a palliative in response to this discontent: it enables those who are ruled, and are therefore politically inferior, to achieve a partial equality with their rulers—their political superiors—by cutting them down to size. Freud alludes to this egalitarian dimension of comedy when he speaks of

> the method of degrading the dignity of individuals by directing attention to the frailties which they share with all humanity, but in particular the dependence of their mental functions on bodily needs.[15] The unmasking is equivalent here to an admonition: such and such a person, who is admired as a demigod, is after all only

> human like you and me. (Freud, 1905c, chap. 7, p. 202)

If it is inevitable that there be an inegalitarian relationship between subject and sovereign, it is at any rate preferable to mitigate that inequality by allowing subjects to joke (and by extension to speak) more freely about and to sovereigns. Here, too, we see that even if all political orders arouse discontent, some political orders (i.e., liberal ones), in which political freedom of speech is permitted, might be preferable to others in that they might arouse significantly less discontent. In this respect, the liberalism of *Jokes* is surprisingly reminiscent of the liberalism of Kant's essay "What Is Enlightenment?": if Kant (1963, pp. 5, 10) contends that the watchword of the liberal state is to "argue . . . but obey!" so does Freud suggest that we "joke, but obey." If we are to be constrained to obey, the least that we can obtain in compensation is the freedom to laugh at those who constrain us to do so.

Freedom of Speech and Catharsis

We have seen indications in both *The Psychopathology of Everyday Life* and *Jokes and their Relation to the Unconscious* that Freud advocates free speech as an outlet for aggression. In order to get a better sense of how such an outlet works and why it is necessary, we must see how Freud's advocacy of free speech is grounded in his psychological theory proper. In this context, one of Freud's earliest works, which he wrote in collaboration with Breuer, is of particular importance. In the work (which is a "preliminary communication" of the findings that they were subsequently to publish in their book-length study of hysteria), Freud and Breuer explain that hysterical symptoms arise when people fail to find an outlet for their aroused emotions:

> The fading of a memory or the losing of its affect depends on various factors. The most important of these is *whether there has been an energetic reaction to the event that provokes an affect.* By "reaction" we here understand the whole class of voluntary and involuntary reflexes—from tears to acts of revenge—in which, as experience shows us, the affects are discharged. If this reaction takes place to a sufficient amount a large part of the affect disappears as a result. Linguistic usage bears witness to this fact of daily observation by such phrases as "to cry oneself out" [*sich ausweinen*], and to "blow off steam" [*sich austoben*, literally, "to rage oneself out"]. If the reaction is suppressed, the affect remains attached to the memory. An injury that has been repaid, even if only in words, is recollected quite differently from one that has had to be accepted. (Freud and Breuer, 1893a, p. 8)

This passage is crucial for an understanding of the psychological underpinnings of Freud's advocacy of free speech because it points to what is distinctively psychoanalytic in this advocacy, as opposed to the more familiar reasons for it that Freud holds in common with earlier liberals. According to Freud, the benefit provided by free speech is primarily psychological and emotional, not political or intellectual: free speech is beneficial because it enables us to "blow off steam" or "to get things off our chest." Freudian theory celebrates free speech, not primarily because it enables the individual or the community to make wise decisions—to choose the most suitable offering at the marketplace of ideas—but because we are psychologically better off when we are able to express our emotions than we are when we must suppress them. The exercise of free speech helps us to feel better more than it helps us to think better or to choose better.[16]

The theme of the emotional benefits that result from "blowing off steam" recurs in a little-known but instructive essay, "Psychopathic Characters on the Stage," which Freud wrote around 1905 and presented to a friend but never published. In the essay, Freud explains how attending theatrical performances can have positive psychological consequences:

> The purpose of drama is to . . . open up sources of pleasure or enjoyment in our emotional life. . . . In this connection the prime factor is unquestionably the process of getting rid of one's own emotions by 'blowing off steam'; and the consequent enjoyment corresponds . . . to the relief produced by a thorough discharge. (Freud, 1942a [1905–6], p. 305)

Spectators achieve this relief vicariously; what they cannot accomplish in deed in their real lives is accomplished for them in speech—the speech of the drama's hero, whose imaginary life is depicted on the stage:

> The spectator is a person who experiences too little, who . . . longs to feel and to act and to arrange things according to his desires—in short, to be a hero. And the playwright and actor enable him to do this by allowing him *to identify himself* with a hero. . . . In these circumstances he can allow himself to enjoy being a "great man," to give way without a qualm to such suppressed impulses as a craving for freedom in religious, political, social and sexual matters, and to "blow off steam" in every direction in the various grand scenes that form part of the life represented on the stage. (Freud, 1942a [1905–6], pp. 305–6)

We learn from Freud's analysis of the drama that one can blow off steam, not only by speaking oneself, but also by listening to others who dare to speak what one fears to say oneself. In either case, however, speech (one's own or another's) is necessary if steam is to be blown off, if the "suppressed ... craving for freedom in religious, political, social and sexual matters" is to be satisfied. Speech both partially compensates us for and provides us with a surrogate for what we are denied in deed. The political and social constraint that is insurmountable in deed is to some extent overcome when we are granted freedom of speech.

Freud begins his essay with a reference to Aristotle's *Poetics*: It "has been assumed since the time of Aristotle [that] the purpose of drama is to arouse 'terror and pity' and so 'to purge the emotions'" (Freud, 1942a [1905–6], p. 305). The reference to "purg[ing] the emotions" is, of course, an allusion to the Aristotelian doctrine of catharsis, with which Freud evidently was familiar. For this reason, it is noteworthy that *catharsis* was the name Freud borrowed from Breuer to designate the form of psychotherapy that Freud later transformed into psychoanalysis.[17] When Anna O., the first analysand, spoke of the "talking cure" (Freud and Breuer, 1895d, chap. 2, sec. 1, p. 30) and when Freud and Breuer (1893a, p. 17) claimed that their "psychotherapeutic procedure . . . brings to an end the operative force of the idea which was not abreacted in the first instance, by allowing its strangulated affect to find a way out through speech," they were referring to catharsis—Freud did not employ the term *psycho-analysis* until 1896 (see Freud, 1896a, p. 151).

As it happens, there is one scholarly interpretation of the Aristotelian doctrine of catharsis that is of particular interest because of its possible connection with the Freudian doctrine of catharsis. This interpretation appears in an essay written by Freud's wife's uncle, Jacob Bernays, which led to "widespread interest in catharsis. . . . [After the essay's republication in 1880] catharsis was one of the most discussed subjects among scholars" (Ellenberger, 1970, p. 484).[18]

In Bernays's presentation, Aristotelian catharsis can almost seem to be a precursor to the catharsis of Breuer and Freud, for Bernays portrays Aristotelian catharsis as a medical phenomenon. He claims that catharsis is "a designation for treatment of a disturbed person, in which the physical influences what is mental; it does not seek to transform or to suppress [*zurückzudrängen*] the disturbing element but to stir it up, urge it forward, and thereby achieve the alleviation of the disturbance" (Bernays, 1880, p. 16). Bernays suggests that catharsis be seen as a process of "excitation of pity and fear," leading to "the alleviating discharge of such . . . emotional states" and argues that this view of catharsis is superior to others in that it "allows the medical metaphor to shine through, as Aristotle did himself in [his reference to catharsis] in the *Politics*" (Bernays, 1880, p. 21).

As we have seen, Freud's advocacy of free speech is clearly reminiscent of the medical understanding of catharsis proposed by Bernays. The passions (or, to use a term employed by Bernays [1880, p. 66] as well as by Freud, [Freud-Breuer, 1893a, p. 8] the "affects") can be "disturbing element[s]." They should be dealt with, not by being "suppress[ed]," but by being "stir[red] . . . up" and "discharge[d]."[19] For Bernays, the spectator of the theatrical piece achieves "the alleviating discharge"; for Freud, people who allow themselves freely to speak their own piece achieve a comparable alleviating discharge.

Freud and Liberal Defenses of Freedom of Speech

As I have argued, Freud's defense of free speech is primarily a psychological one. Nevertheless, there are important political and sociological presuppositions underlying it that should be made clear because they point to an important affinity between Freud and an element within the liberal tradition.

We have seen that Freud advocates free speech because of his belief in the utility of blowing off steam. We have not yet really considered, however, why it is that steam must be blown off. This question is implicitly answered in those passages in *Psychopathology* and *Jokes* that prefigure the critique of social life that Freud was later to elaborate in *Civilization and its Discontents*: it is already apparent in *Psychopathology* and *Jokes* that the "steam" that must be blown off results from the friction generated by social interaction. Society and government must restrict people to keep them from harming one another, but this restriction itself is perceived by people as a kind of harm. That being the case, the liberalism evident in Freud's advocacy of free speech is intelligible as a partial response to the antisocial character of human beings that he discusses in *Civilization*: it is in the interest of both government and subjects to limit the government's restrictions upon its subjects, to the extent it is possible and safe to do so. People must be prevented from hurling spears at one another because spears cause physical damage; since people arguably do little real damage to one another by hurling words of abuse, a prudent government might decide to grant people an outlet for their aggressiveness (at the same time lessening their frustration at being governed) by granting them freedom of speech—by freeing them to hurl words of abuse at one another. In a way, Freud's view is expressed in the words of the childhood chant "Sticks and stones may break my bones, but words can never harm me." [20] In view of Freud's belief that childhood in some way reveals the deepest truth about human nature, it is altogether appropriate that this slogan's sentiment should underlie his psychoanalytic advocacy of free speech.[21]

In different forms, arguments like this can be found in a number of liberal thinkers. Hobbes (1972, chap. 13, sec. 17) speaks of "the harmless . . . liberty of subjects." For Hobbes, as for Freud, subjects must be denied the liberty that leads them to harm one another but, in compensation, they can and should be granted "harmless liberty." In order for people to enjoy this harmless liberty, "it is necessary that there be infinite cases which are neither commanded nor prohibited, but every man may either do or not do them as he lists himself. . . . The more is left undetermined by the laws, the more liberty [subjects] enjoy." To secure subjects their harmless liberty, Hobbes (1972, chap. 13, sec. 15) counsels governments against excessive legislation, wherein "such things are forbidden as reason of itself prohibits not of necessity."

Hobbes's argument is, of course, about liberty generally not about freedom of speech specifically. A version of his argument is made more specifically relevant to the issue of freedom of speech by a later liberal, Adam Smith. Smith (1937, bk. 5, chap. 1, pt. 1, p. 668) writes that "that degree of liberty which approaches to licentiousness can be tolerated only in countries where the sovereign is secured by a well-regulated standing army" and that "the rudest, the most groundless, and the most licentious remonstrances . . . can give little disturbance . . . to a sovereign . . . supported . . . by a well-regulated standing army." For this reason, Smith contends, supporters of personal freedoms (such as freedom of speech) should also support the strength and power of the sovereign.[22] Smith presents freedom of speech as a benefit that subjects can enjoy only when they live under a government strong enough to keep the peace securely. Thus, Hobbes implies and Smith states that people can be free to hurl words of abuse at one another only when their sovereign is in sufficient control of their actions to prevent them from hurling spears at one another (and at him). Freud amplifies their argument by asserting that permission to hurl words provides an additional social benefit: it compensates the subjects, by enabling them to blow off steam, for the frustration they feel as a result of being prevented from freely employing their spears.

The premises behind all of these arguments are that obedience is a condition that people resent, that they can be compelled to obey, but that it is prudent to avoid employing more coercion than is strictly necessary. It follows from these premises that people can perhaps be coaxed to obey in some instances by not being compelled to obey in others. Thus, in order to secure the obedience of their subjects when they need to have it, governments should take care not to insist upon the obedience of their subjects when they do not need to have it. This conclusion was well stated in 1841 by the liberal historian Macaulay (1907, p. 419): "It may be laid down as a universal rule that a government which attempts more than it ought will perform less." Freud's advocacy of free speech rests upon a similar assumption: that government should not attempt altogether to repress human aggressiveness but should instead allow it to be harmlessly expressed in speech.[23]

Freud and the classical liberals agree on the advisability of freeing speech from political controls, but there is an important respect in which his attitude toward speech differs from theirs. It is true that some liberals share Freud's view that free speech is defensible in part as a safety valve that permits people to express their frustrations harmlessly, but this is far from being the only defense of free speech that liberals historically have offered. Liberals have celebrated free speech not only because it does no harm but also because it can do much political good; they have contended that free political debate enables the political alternatives to emerge more clearly, which enables the political community to choose more wisely among them. For this reason, liberals like John Stuart Mill have praised speech for its capacity to arouse people to action and to transform the political world.

This aspect of the liberal defense of free speech has no analogue in the psychoanalytic defense of free speech that we have been considering. For this reason, Freud's defense of free speech is in some respects also a critique of speech: insofar as Freud advocates freeing the everyday speech of ordinary political and social encounters to create a safety valve for the aggressiveness that such conflictual encounters engender, he depreciates the significance of free speech by being oblivious to its capacity to transform the political and social world.

A Political Critique of Freud

Freud's critique of political speech is apparent in his understanding of speech as an *Ersatz* for a deed; speaking is a "less adequate" means for working off an affect than is doing (Freud, 1893h, p. 36; cf. the reference to an injury being repaid, "*even* if *only* in words" [emphasis added] in Freud and Breuer, 1893a, p. 8, quoted above). It is preferable to complain about an intolerable situation than to ignore it or to pretend that it is tolerable, but to complain about it is still very different from and far inferior to the taking of steps actually to alter and improve the situation. A quotation from Lessing employed by Freud suggests the problem I am addressing here: "Not all are free who mock their chains" (Freud, 1905c, chap. 3, p. 92). It is, again, better to mock one's chains than to celebrate one's confinement, but it is better still to break one's chains and to end one's confinement.

To complain about one's confinement truly liberates insofar as it leads to deeds that end confinement. Insofar as it serves only as an *Ersatz* for ending confinement, complaining may increase that confinement by making it seem less intolerable. For this reason, Freud is somewhat ambivalent about free speech, and for this reason, as I have already suggested, Freud's favorable assessment of free speech prefigures Marcuse's condemnation of repressively tolerant free speech. To make this point differently, Freud, like Marx (whose intellectual career also began with a condemnation of censorship),[24] wants primarily to "change" the world: if Marx fears that speech can assist us only to "understand" the world (see the last of the "Theses on Feuerbach," in Marx and Engels, 1978, p. 145), Freud fears that it can assist us only to express our resentment of it. Accordingly, in Freud's view as in Marx's, people "are in no way combating the real existing world when they are merely combating the phrases of this world" (*The German Ideology,* in Marx and Engels, 1978, p. 149).

The affinity between the Freudian defense and the Marcusean critique of free speech in itself suggests the former is in some way deficient. It is deficient because an adequate defense of free speech must be Millian—that is, must celebrate the capacity of free speech to improve political deliberation—as well as psychoanalytic. To be fully defensible, speech must be thought to be more than a substitute for action in the external world; it must also be thought to be a preparation for action in the external world, in fact itself a form of action in the external world.

Freud is, of course, quite correct to note that words are often not followed by deeds—that in fact a word often is nothing but an *Ersatz* for a deed. Nevertheless, the view of the word as an inferior substitute for the sword is inadequate. It is particularly inadequate with respect to politics and still more particularly so with respect to liberal politics, for in politics the word does not so much replace the sword (as the means through which we take out our aggression on others) as it directs the employment of the sword. In all regimes (and particularly deliberative regimes) it is through words that political figures determine how the sword is to be employed, who is to employ it, and against whom it is to be employed. This is to say that speech serves not only as a vehicle for the expression of emotion but also (and more importantly) as the medium in which political decisions are made; an adequate defense of free speech must recognize this deliberative function of speech as well as the expressive function emphasized by Freud.

Nor is this the only respect in which Freud's defense of free speech is somewhat inapplicable to politics. At best, his theory suggests an ideal (speakers' ability freely and unreservedly to give vent to their emotions) that even good political communities (and especially, in some measure, good political communities) can and should never approach.

I say this because the completely candid airing of one's views, however therapeutic it may be, is out of place in politics.[25] Language itself suggests this in the etymological relationship between "politics" and "polite," in the phrase "an impolitic remark." It is worth recalling the Swiss Professor N., who is said by Freud's informant to have uttered the neologism *moche* in part in reaction to his "anxiety about committing a political indiscretion" (Freud, 1901b, chap. 5, p. 73); in his case and in many others, to say what one would like to say is to say something that politically one ought not to say.

Politics requires discretion for the very same reasons that social life in general does, and there need be nothing hypocritical about this requisite discretion. In politics as in social life generally, words are intended to have consequences for the behavior of others; thus, speakers weigh their words more or less carefully and do not utter the first thing that comes into their minds out of consideration for the possible consequences of their words upon others. With respect to politics, better political communities, in which statesmen are more concerned with the welfare and the political views of those whom they rule, might in fact require their statesmen to be more cautious and circumspect in their political speech than would worse political communities.

Even if it is true that good political communities enable their citizens freely to voice the truth as they see it, political figures must nevertheless exercise caution and circumspection in the way they choose to express the truth. Political success requires one to have supporters, and political rhetoric must be employed in order to attain and to maintain support. Simply voicing the truth is insufficient; the statesman must also consider how to voice what he takes to be the truth—how to express it so as to make it most persuasive to his listeners. Thus, politics necessarily places constraints on self-expression: the aim of political speakers is not primarily to express their views but to induce others to accept them.

A political defense of free speech must therefore take into account the effect of speech upon its listeners. Freud's defense is too apolitical in that it emphasizes the effect upon the speaker.[26] In all political circumstances, consideration of the listeners will constrain the speaker. In some respects, liberal democracies may constrain the speaker less than other governments, but even liberal democracies cannot entirely make possible the sort of therapeutic blowing off of steam that Freud emphasizes in his advocacy of free speech.[27] To advocate free speech on psychoanalytic grounds, in other words, is perhaps less to advocate free government than it is to advocate no government.

There is something solipsistic about the psychoanalytic defense of freedom of ordinary political and social speech, in which the goal of speech is less to persuade the listener than to heal the speaker. By emphasizing the emotional value of using words to express one's feelings, Freud deemphasizes the political value of using words to persuade others to accept one's ideas; thus the psychoanalytic defense of free speech can be said to be excessively "expressive" (of the speaker) and insufficiently "impressive" (i.e., intended for the audience). If free speech is reduced to getting things off one's chest, one can too easily forget that its purpose is primarily to get ideas out of one's head and, more importantly, to get them into the heads of others.

In this respect, it is interesting to consider what has been called "one of the more curious developments in recent intellectual history[:] . . . the metamorphosis of freedom of speech and press into freedom of expression *tout court*. . . . The glide from 'the freedom of speech' . . . to 'freedom of expression' has been a significant one" (Canavan, 1984, pp. xi–xii). Following Canavan, I suggest this metamorphosis is significant because it points to a decreasing concern with the specifically political character that speech potentially has. Many forms of self-expression are not and cannot be political (e.g., playing musical instruments) because they are not and cannot be used for purposes of persuasion. Speech can be persuasive (although, of course, it need not be intended to be); for that reason, freedom of speech (unlike the freedom of other forms of self-expression) is politically essential if self-government is to be ensured. It may be beneficial for a speaker to express his emotions; but for him to do so is very different and very much less important politically than it is for him to express his ideas persuasively and coherently.

The contemporary shift from freedom of speech to freedom of expression, which results from the presupposition of the essential equivalence of all the myriad forms of self-expression, is significantly prefigured in Freud's psychoanalytic understanding of speech; this leads him to neglect the possible political significance of speech. More importantly, his emphasis upon the affective as opposed to the intellectual significance of words leads him, in a sense, to depreciate the significance of speech altogether.[28] Speech is not always needed to express emotions—grunts and grimaces can often accomplish this just as well. Speech is, however, needed to express ideas. To the extent that Freud himself deemphasizes this, one can almost say there are Freudian grounds for the attempt to a contemporary psychotherapist to outdo Freud by suggesting "the primal scream" as a suitable substitute in the psychotherapeutic situation for the lengthy, hyperverbal exchange with an analyst.[29]

Thus, although Freudian psychoanalysis provides a defense of free speech, it is a partial defense that does not defend it for all of the right reasons. In defending free speech, it may be helpful to recall that Aristotle speaks in the *Poetics* of the utility of purging our emotions; it is still more helpful to recall that he also speaks, in the *Rhetoric*, of the utility of employing our emotions to persuade our listeners to accept our political ideas. It is also helpful, in defending free speech, to recognize its role in securing free government and to recall the benefits that can result from free government as well as (and as opposed to) the constraints imposed by all government.

Notes

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1. See Eastman (1942, p. 270): "'What are you politically?' I asked. [Freud answered;] 'Politically I am just nothing.'"

2. See Freud's letter to Arnold Zweig of 26 November 1930: "I remain a liberal of the old school" (Freud and Zweig, 1970, p. 21).

3. It is important to realize, however, that the analogy is only a partial one. It is true that both psychoanalytic patients and average healthy social beings benefit from "telling things" and from the ensuing discharge of disturbing emotions; but the patients benefit more because they can tell things to an analyst, who is trained (as ordinary social interlocutors are not) to interpret their statements and to assist them in understanding and dealing with their disturbing emotions. As will become clear below, the ordinary members of society receive a catharsis from speaking freely; they do not, of course, receive a psychoanalysis.

4. See *Civilization and its Discontents*: "Men are not gentle creatures who want to be loved, and who at the most can defend themselves if they are attacked; they are, on the contrary, creatures among whose instinctual endowments is to be reckoned a powerful share of aggressiveness. As a result, their neighbour is for them not only a potential helper or sexual object, but also someone who tempts them to satisfy their aggressiveness on him" (Freud, 1930a, chap. 5, p. 111).

5. See "Instincts and Their Vicissitudes": "Hate, as a relation to objects, is older than love. It derives

from the narcissistic ego's primordial repudiation of the external world with its outpouring of stimuli" (Freud, 1915c, p. 139).

6. See, "'Civilized' Sexual Morality and Modern Nervous Illness," where Freud (1908d, p. 197) criticizes the "well-behaved weaklings who . . . become lost in the great mass of people that tends to follow, unwillingly, the leads given by strong individuals."

7. Freud's German word, correctly translated as "candid," is *freimütiger* (literally, "free-spirited"). The translation conceals an allusion to freedom in the German text: Herr A.'s sons, who might speak freely and reveal their Jewishness, are free in a way that Herr A. himself is not. Note also that the sons' *Freimütigkeit* is the opposite of the "fainthearted-ness" (*Schwachmütigkeit*) to which, as we have seen, Freud himself "like[sé to avoid concessions."

8. Freud's German word, translated as "more frankly," is again *freimütiger*, and again the translation conceals an allusion to freedom in the German, which suggests that the Austrians were freer when they could openly complain about their German allies than they were when they could not (cf. note 7, above).

9. Compare this passage with that on pages 108–16 of *Civilization and Its Discontents* (Freud, 1930a, chap. 5), in which Freud again discusses the impracticability of the injunction to "love thine enemies" (p. 110) and again contends that "a comparatively small cultural group . . . allow[s the aggressive instinct] an outlet in the form of hostility against invaders" (p. 114).

10. According to Strachey's editorial note, "it seems likely that Herr N. was Josef Unger (1828–1913), Professor of Jurisprudence and from 1881 President of the Supreme Court" (Freud, 1905c, chap. 2, p. 22).

11. Note that the physician in Freud's joke is always referred to as "the professor."

12. See Jones (1953–57, vol. 1, pp. 338–41). Freud's promotion was passed over from 1897 through 1901 and was finally granted in 1902.

13. For Freud's rejection of monarchy, see also Schorske (1981, p. 189), who, in listing "the political values [Freud] retained all his life," speaks of Freud's "contempt for royalty and aristocracy (in 1873 as a senior in the Gymnasium Freud had proudly refused to doff his hat to the emperor)."

14. In an editorial note, Strachey explains that Serenissimus was "the name conventionally given to Royal Personages by comic periodicals under the German Empire" (Freud, 1905c, chap. 2, p. 68).

15. This statement is of additional interest in that it points to an egalitarian thrust underlying Freud's own work: few figures, after all, have stressed "the dependence of . . . mental functions on bodily needs" more than Freud.

16. In this context, it is instructive to compare John Stuart Mill with Freud. In *On Liberty* Mill, like Freud, argues that people benefit psychologically from having and from exercising freedom of speech, but Mill's argument emphasizes the free development of the intellect, whereas Freud's psychoanalytic argument emphasizes the free play to be given to the emotions: "The greatest harm done is to those . . . whose whole *mental development* is cramped, and their *reason* cowed, by the fear of heresy. Who can compute what the world loses in the multitude of promising *intellects* combined with timid characters, who dare not follow out any bold, vigorous, independent train of *thought*?" (Mill, 1961, p. 284, emphasis added). This contrast does not, of course, apply to Freud's nonpsychoanalytic or Millian argument in behalf of intellectual freedom, which is particularly evident in *The Future of an Illusion* (see discussion above).

17. For an account of the transformation of catharsis into psychoanalysis, see *An Autobiographical Study* (Freud, 1925d [1924], chaps. 2–3, pp. 19–31).

18. There is good reason to believe Freud was both generally familiar with Aristotle's *Poetics* and specifically acquainted with Bernays's interpretation of it. Freud's library contained two translations of the *Poetics* out of only five classical Greek works (Trosman and Simmons, 1973, p. 679). Furthermore, one of the translations contains an essay in which "the Jacob Bernays interpretation of the Aristotelian theory of catharsis is in the main taken as a basis" (von Berger, 1897, p. 73); the essay goes on to assert that "the cathartic treatment of hysteria, which the physicians Josef Breuer and Sigmund Freud have described, is very suitable for making the cathartic effect of tragedy understandable" (von Berger, 1897, p. 81). The translation to which the essay is attached is by an acquaintance of Freud, the noted classicist Theodor Gomperz, who hired Freud to translate some of John Stuart Mill's works into German and whose wife, who at one time was a patient of Freud's, attempted to use her influence to secure for Freud the promotion that he desired at the University of Vienna (see Jones, 1953–57, vol. 1, pp. 55–56, 340, and discussion above). In light of Freud's acquaintance with Gomperz, I think it particularly unlikely that he should have been unaware of the views of the eminent classicist whose niece he had married.

19. Note that Freud's word, translated as "repress" (*verdrängen*), shares a common root with Bernays's word that I have translated as "suppress" (*zurückdrängen*), differing only in its employment of another prefix.

20. To say that words do little harm is, of course, to tell only half of Freud's story: it is true that abusive words do not harm their recipients as much as physical attacks do, but it is equally true (and at least as important) that abusive words do help their speakers (though not as much as physically attacking their opponents might help them).

21. The Freudian argument that I have reconstructed here on behalf of free speech is, however, turned on its head by Marcuse (1969). For Freud, free speech is a compensation granted to subjects because the necessity that subjects obey is taken for granted (as is the subjects' perception that obedience is unpleasant). For Marcuse, since obedience ought not to be taken for granted, free speech is problematical: subjects should not settle for the compensation that the right to speak freely offers them but instead should cease to obey; by ceasing to blow off steam and ceasing to obey, subjects could and should bring the "[steam] engines of repression" to a halt. Marcuse (1969, p. 115) criticizes "nonconformity and letting-go in ways which leave the real engines of repression in the society entirely intact, which even strengthen these engines by substituting the satisfactions of private and personal rebellion for a more than private and personal, and therefore more authentic, opposition." I return to the connection between the arguments of Freud and Marcuse below.

22. The same argument is made by Kant (1963, p. 10): "Only one who . . . has a numerous and well-disciplined army to assure public peace, can say: 'Argue as much as you will, and about what you will, only obey!'"

23. It is noteworthy that one of Freud's judgments about neuroses makes the same point in a medical context that Macaulay makes in a political context: "Experience teaches us that for most people there is a limit beyond which their constitution cannot comply with the demands of civilization. All who wish to be more noble-minded than their constitution allows fall victims to neurosis; they would have been more healthy if it could have been possible for them to be less good" (Freud, 1908d, p. 91).

24. See Cuddihy (1974, p. 121): "In February 1843 the first political article Marx ever wrote, on censorship, appeared in the Swiss magazine *Anekdota*. Significantly, Freud's first great work appearing at the century's end, *The Interpretation of Dreams*, was also to deal with censorship."

25. In this context, Freud's description of his unsuccessful attempt to psychoanalyze a political figure is of some interest, suggesting as it does the potential conflict between the psychoanalytic insistence on candor and the prudence demanded by politics: "I once treated a high official who was bound by his oath of office not to communicate certain things because they were state secrets, and the analysis came to grief as a consequence of this restriction" (Freud, 1913c, p. 136).

26. I should make it clear that I do not assert that Freud personally was altogether unaware of the considerations I raise here or that he would altogether have denied their relevance; recall his "recog[nition of the] social usefulness" of "conventional expressions of sympathy" (Freud, 1901b, chap. 7, p. 155). What I do assert is that Freud's psychoanalytic defense of free speech is partial at best; it is inadequate to the considerable extent that it fails sufficiently to emphasize the social and persuasive character of speech.

27. See Podhoretz (1984, p. 56): "In a time when nothing is concealed, whether in private life or public, when all the formerly dirty little secrets are being uncovered, when all the 'closets' are being emptied, when what used to be covert, whether in Nicaragua or the bedroom, is now exposed to the full glare of publicity—when, in short, the whole of existence seems to be regulated by one gigantic sunshine law—we have no reason to be surprised if the memoirs of public officials are less and less governed by the decorous inhibitions of the past. Nevertheless I for one am still—though probably not for much longer—at least a little surprised at how outspoken public officials are becoming." I would argue that the phenomenon to which Podhoretz here calls attention is itself a testimony to the impact upon American society of a vulgarization of the Freudian ethics of honesty and self-expression.

28. See, for example, *Delusions and Dreams in Jensen's "Gradiva"*: "We remain on the surface so long as we are dealing only with memories and ideas. What is *alone* of value in mental life is rather the feelings. *No* mental forces are significant unless they arouse feelings" (Freud, 1907a, chap. 2, pp. 48–49, emphasis added).

29. See Janov (1970, p. 91): "Though the scream is the most usual reaction, it is neither the sole nor the perennial response to sudden vulnerability to Pain. Some people moan, groan, writhe, and thrash about." They do not, it appears, need to speak. See also Janov's (1970, p. 388) distinction between "conventional [word-oriented] psychotherapy," which "has been acceptable to middle-class intellectuals," and "the Primal view."

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REGULATORY ENFORCEMENT IN A FEDERALIST SYSTEM

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Federal agencies integrate federal, state, and local political demands at the operational level of service delivery. They balance conflicting political demands and task requirements as they attempt to develop feasible enforcement routines capable of attracting support and resources in multiple arenas without undermining central support for budgetary resources and statutory authority. Our regression analysis of annual enforcement data from 1976 through 1983 for all 50 states indicates that even the relatively isolated enforcement procedures of the Occupational Safety and Health Administration respond significantly to state-level political and task differences. Enforcement activities responded most consistently to daily enforcement contacts with interest groups. State differences in task conditions—particularly workplace accident and unemployment rates—also elicited instrumental responses, while differences in the party and ideology of elected officials elicited more symbolic actions. State agencies, with their smaller size and greater flexibility, were even more responsive than the federal agency to political and task differences. This integrative function of bureaucracy needs further attention in democratic theory.

Two contrasting images of public bureaucracy have dominated the study of federal agencies, particularly those responsible for regulatory enforcement. One image focuses on central controls over agency structure, tasks, and budgets and emphasizes the importance of elected officials and pressure groups in determining bureaucratic behavior. The other image, more dominant in the field of public administration, focuses on the nature of the policy task and the organizational characteristics of a bureaucracy and notes that organizational imperatives dominate the actions of public bureaucracies. This analysis contributes to recent efforts to integrate and expand these images in order to understand the dynamic role of federal agencies in a disjointed, incremental, federalist system (Bendor, Taylor, and Van Gaalen, 1985; Chubb, 1985, Moe, 1985a). Although the argument applies more broadly, this research is focused on regulatory enforcement and presents evidence from our study of the enforcement activities of the Occupational Safety and Health Administration (OSHA).

Both images are rooted in the classical distinction between political and administrative questions that lies at the base of democratic theory. Political officials acquire the authority to settle political questions through the electoral process, while bureaucrats are delegated authority to settle administrative questions because of

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their technical expertise. Although this distinction has long been discredited, current political science studies still reflect these images largely because of specialization that focuses attention on either electoral or bureaucratic institutions, but seldom on both.

Studies of political institutions note that the president, Congress, and courts not only determine the general tasks and organizational form of regulatory agencies but also control enforcement activities directly through various oversight powers. Even if not regularly used, potential controls are presumed sufficient to make bureaucrats responsive to central political changes (McCubbins, 1985; McCubbins and Schwartz, 1984; Weingast and Moran, 1983). Quantitative empirical studies have provided evidence about bureaucratic responsiveness to a major political realignment in congressional committees, (Weingast and Moran, 1983; but see Moe, 1985b), to the periodic realignments corresponding to different presidential regimes (Moe, 1982), and to more incremental changes in several federal institutions (Chubb, 1985; Moe, 1985a). Party alignments of the president and Congress have been argued to affect regulatory budgets (Kemp, 1985). A few state-level studies have indicated that state bureaucracies are even more responsive than the federal bureaucracy to political influences, particularly from business groups (Marlow, 1981; Marvel, 1982; Williams and Matheny, 1984), and one city-level study found responsiveness to patronage institutions but not to organized interest groups (Jones, 1981).

Studies focusing primarily on implementation by administrative agencies, on the other hand, tend to view elected officials as relatively distant, minor participants in the daily battles within the bureaucracy that determine policy impacts. The specialized technical, legal, and administrative knowledge, the authority to make specific decisions, the control over important streams of information, the reputation for revenge and reliability, the lack of electoral incentives, and the staying power of bureaucratic participants give administrative agencies considerable resources to thwart specific demands from elected officials (Allison, 1971; Bardach, 1977; Neustadt, 1980). The influence of interest groups over regulatory agencies is more widely acknowledged, particularly because they are considered more persistent and adept at bureaucratic politics than elected officials (Bernstein, 1955; Lowi, 1969; Mitnick, 1980; Noll and Owen, 1983). However, their ability to influence enforcement (as opposed to rulemaking) decisions—particularly in regulatory agencies like OSHA, which processes large numbers of cases through a complex sequence of specialized routines—is limited by case processing requirements (Diver, 1980) as well as by legal restrictions from the Administrative Procedures Act and related court decisions designed to limit the ability of powerful groups to influence individual enforcement actions. Furthermore, in conflict-ridden political environments such as OSHA's (McCaffrey, 1982; Mendeloff, 1979), conflicting pressures from contending groups provide added incentives for enforcement officials to guard the image of impartiality and avoid even the appearance of responding to enforcement requests from individual elected officials.

Thus, although elected officials and interest groups have been shown to influence aggregate national enforcement policies, the extent of their control over the details of enforcement and the variance of enforcement actions across geographic regions are less well established. Studies of bureaucratic output in other contexts, particularly in the delivery of urban services, have found the distribution of output to be determined not by political influence but by routines established by the agency to perform its assigned tasks (Jones, Greenburg, Kaufman, and Drew, 1977; Levy, Meltsner, and Wildavsky, 1974; Lineberry, 1977). Similarly, case studies of enforcement agencies have tended to discount political influences over bureaucratic activities, explaining agency behavior in terms of the professionalism of enforcement staffs, established work routines, task requirements for detecting and prosecuting violations, attitudes towards regulated firms, and more idiosyncratic behavior of inspectors (Carson, 1970; Diver, 1980; Hawkins, 1984; Hawkins and Thomas, 1984; Kagan, 1978; Katzman, 1981; Kelman, 1982; Nivola, 1976; Shover, Lynxwiler, and Clelland, 1983; Weaver, 1977).

Explanations of enforcement behavior that emphasize internal factors complement political explanations to the extent that internally induced variations in enforcement output come about in response to different task conditions for statutory tasks. Indeed, responsiveness to statutory objectives requires the observed role of expertise and professional beliefs in establishing work routines (Bardach and Kagan, 1982; Kagan, 1978; Kagan and Scholz, 1984; Kaufman, 1960; Kelman, 1982; Shover et al., 1983) in order to achieve statutory goals. Other regulatory studies reflect a common perception of bureaucracy (Niskanen, 1971) that internal factors reflecting bureaucratic interests, bureaucratic cultures, and staff idiosyncrasies diminish the agency's concern with statutory goals, thereby reducing the likelihood of both task and political responsiveness. (Katzman, 1981; Nivola, 1976; Silbey, 1984; Weaver, 1977). Furthermore, as implementation studies have made clear, political demands from various local, state, and federal institutions are frequently in conflict with statutory goals, and the statutory goals themselves seldom lead to unambiguous definitions of appropriate tasks. The degree to which federal enforcement is or should be altered to cater to local variation in task conditions or political demands remains ambiguous in descriptive as well as prescriptive theory.

Our study of OSHA probes the degree to which its enforcement activities vary in response to political and task differences among states. OSHA provides an excellent subject because it undertakes or supervises extensive enforcement actions of a relatively similar nature in all states, because its regulations have remained relatively stable during the period of study, and because its inspectors had developed a reputation in its early years of being legalistic and unresponsive to important task factors (Bardach and Kagan, 1982), making it a difficult test case for enforcement responsiveness. Because we are primarily concerned with the responsiveness of established enforcement agencies, we included state-level enforcement data from 1976 to 1983, the period after the problems of OSHA's formative years had diminished. During this period few new laws and organizational tasks were imposed on OSHA, and yet there were differences among states and considerable changes in political and task factors expected to affect enforcement (Centaur Associates, 1985). Before presenting the results of our analysis, we will discuss hypotheses concerning (1) the political factors likely to influence OSHA, (2) the task factors likely to influence OSHA, (3) the instrumental and symbolic enforcement responses available to OSHA, and (4) the comparative responsiveness of federal and state agencies that enforce OSHA regulations.

HYPOTHESIS 1. Agencies respond to changes in multiple political environments.

The primary hypothesis to be tested in this study is that bureaucracies respond rationally to multiple political and task signals from their environment, altering activities in different time periods and geographic regions according to these signals. Given the complex structure of American politics and the multiple paths of influence over government agencies, however, it is not at all clear which political signals the agency should be expected to follow. A narrow legalistic view of democratic authority would presumably argue that only federal elected officials should influence federal bureaucracies like OSHA, and only through legitimate policy channels. Thus, the party affiliation and ideological inclination of a state's congressional contingent should have an overall impact on aggregate national enforcement levels but not on the particular level of enforcement in the given state. Once the national policy is set, differences in enforcement levels among states should reflect only differences in task conditions. With few exceptions (Chubb, 1985), research has inadvertently supported this narrow view because of the primary focus on national-level enforcement responsiveness.

A pragmatic agency, however, is likely to respond to the more subtle concerns of congressmen for their particular districts by initiating more intensive enforcement efforts in areas and industries where congressmen hope to maintain strong labor support and by developing more cooperative enforcement programs in areas and industries where business backing is important to congressmen. Such variation in enforcement may, in some instances, reflect conscious strategies by agency leadership to gain or maintain support from particular elected officials but may also reflect the local enforcement official's inclination to take advantage of an elected official's willingness to provide local leadership and implementation resources. The first strategy attempts to increase the agency's formal resources (budgets and statutory authority) while the second attempts to increase external resources provided by outside groups. Of course, the second strategy is aimed not only at elected officials but also at interest groups, whose resources are required to supplement formal resources (Bardach, 1977; Pressman and Wildavsky, 1976; Selznick, 1966), particularly given the chronic insufficiency of the enforcement resources allocated to OSHA and other regulatory agencies to establish a credible enforcement threat (McCaffrey, 1982; Mendeloff, 1979; Smith, 1976). To the extent that required local resources are more problematic than formal resources, the cumulative effect of local electoral and interest group networks would be expected to have a greater impact on national enforcement activities than national political alignments would have. Indeed, federal agencies may, of necessity, seek local alliances required for program success, even at the cost of alienating some congressional support (Selznick, 1966).

Local interest group support can be even more important to enforcement officials than the support of local elected officials because their members can directly help or hinder enforcement. Enforcement agencies routinely meet with affected organizations at various levels to discuss inspection procedures, conflicting interpretations of regulations, and other enforcement problems. In addition, the act establishing OSHA, like most other regulatory enforcement legislation, provides several points at which individuals from affected business and labor organizations directly affect the enforcement process. Labor representatives, for example, can increase the likelihood that violations will be found and penalized by filing complaints with OSHA alleging violations in their workplace and by accompanying OSHA officials during inspections. Although all workers have the right to file complaints anonymously, complaints are most likely to be filed when systematically encouraged by active unions.

Business, on the other hand, can increase the cost of enforcement actions—thus reducing the level of enforcement activities—in many ways, particularly by appealing citations to the special appellate body, the OSHA Review Commission. As appeals increase, inspectors must spend more time carefully documenting citations and preparing testimony. In addition, appeals boards almost automatically reduce fines and dismiss some citations: for example, during one period in California, penalties were cut by an average of 50%, according to CAL-OSHA officials. Again, appeals by individual businesses are more likely when encouraged by active trade associations.

If agency responses are assumed to be rational, the signals to which OSHA responds provide some clues about the relative importance of formal and informal resources to the agency. To the extent that local resources are more important for enforcement than formal resources, direct actions by interest groups and local election networks should have a measurable impact on enforcement activities. Even state legislators and governors are then likely to have some influence over federal policies. To the extent that central resources are more important, only political forces filtered through Congress, relevant congressional committees, and the president should affect the distribution of enforcement activities, and task conditions should be a primary cause for enforcement variation between states.

Political data for the OSHA study was gathered to provide a preliminary mapping of how responsive enforcement is to federal and state elected officials and to interest groups. To control for and assess the impact of the Ford, Carter, and Reagan administrations on national enforcement levels during the period of the study, we include dummy variables for presidential administrations. To measure the political effect of a state's congressional contingent, we assume that congressmen who vote more frequently for prolabor positions in Congress are also more likely to seek higher levels of enforcement in the district. We therefore calculate a prolabor ideological index for each state contingent based on the mean difference between trade union Committee on Political Education (COPE) (prolabor voting) and National Association of Manufacturers (NAM) (probusiness voting) scores for all senators and congressmen from the state.

At the state level, ideological indicators like COPE and NAM scores are not consistently available. Given the prolabor nature of OSHA enforcement and the long-standing alliances of business with the Republicans and labor with the Democrats, we assume that Democratic governors and legislators are more likely to seek higher levels of enforcement. Although this simplification does not do justice to the diversity of partisan alignments in state politics, partisanship is used frequently in a similar manner in state policy studies (Cnudde and McCrone, 1969; Dye, 1969; Jennings, 1979; Morehouse, 1981; Sharkansky and Hofferbert, 1969). Thus, governors are represented by a dummy variable valued at 0 for Republicans and 1 for Democrats, and state legislatures are represented by the percentage of Democrats elected to the combined legislative bodies in the state.

For interest groups, we expect the level of enforcement activities to vary directly with the support provided by labor and inversely with the resistance offered by business. We include the number of worker complaints per 1,000 industrial workers filed with OSHA as the best available measure of the direct activity of labor to influence enforcement but unfortunately could not use the business equivalent—the number of citations appealed by firms—because of missing data.[1]

In summary, we expect that OSHA enforcement will be most vigorous during periods of and in states with high levels of labor complaints and more Democratic and prolabor elected officials than probusiness and Republican ones, although we are uncertain whether responsiveness will be more significant for legislative or executive officials at the state or federal level.

HYPOTHESIS 2. *Agencies respond to changes in task environments.*

Economic models of enforcement note that enforcement efficiency requires the shifting of resources among enforcement arenas with different task characteristics until marginal returns in each area are equal (Stigler, 1970). Thus, in order to obtain maximum political credit for implementing the formal tasks assigned by central political authorities with a given budget, an agency would vary its enforcement activities over time in different states to reflect differences in task-related conditions affecting the return on enforcement activities.

However, organizational and political problems limit an agency's ability to adapt to changes in task-related factors. Implementation research has emphasized the difficulty agencies face in assembling and coordinating effective routines to accomplish any task (Bardach, 1977), even for modest policy goals in relatively tranquil political environments (Pressman and Wildavsky, 1976). Once effective routines have been established, organizational rationality requires that they be standardized, that variation be minimized, and that core technologies (in OSHA, for producing and processing citations) be buffered from environmental fluctuations (Thompson, 1967), all of which reduce the variation to be expected in enforcement due to task differences. Thus, task responsiveness becomes diluted by organizational necessities.

Conflicting political signals further reduce an agency's potential task responsiveness. First, the centrifugal force of local political pressures and the bureaucratic interests of local operating units pose a problem that federal agencies frequently solve through standardizing procedures and output beyond the requirements for organizational efficiency (Kaufman, 1960). Professional standards, acceptable measures of task output and of task-related differences in local conditions, and agreement on the causal relationships between agency activities and outcomes (e.g., between OSHA's enforcement efforts and the reduction of risk) generally aid agencies in preserving the discretion of local operating units to vary output in response to local conditions. In compact urban agencies delivering standard services like fire protection and road maintenance, for example, technical decision routines have been found to determine the difference in level of service among an agency's service areas. Political differences between areas had less of an effect for these professionalized services (Levy et al., 1974) but a greater effect on less-professionalized urban bureaucracies; bureaucratic decision rules had less of an effect on less-professionalized urban bureaucracies (Jones, 1981; Jones et al., 1977). Because federal agencies like OSHA generally have less-developed professionalism, less-established technologies, greater organizational problems imposed by broader mandates and jurisdictions, and more complex political environments than urban agencies, task responsiveness becomes more questionable. For OSHA in particular, the contentiousness of the political environment and the difficulty in establishing appropriate task-related justifications for different levels of enforcement decrease the likelihood of task-related responsiveness.

Two testable decision rules about the efficient use of scarce resources emerged from interviews with enforcement officials as potential indicators of task responsiveness for OSHA. First, we expect that enforcement activities will be highest where industrial accident rates are highest because enforcement professionals tend to believe that enforcement resources are most effectively deployed where high accident rates indicate that safety problems are more numerous. Accident rates affect inspectors, who look at past accident records of firms during inspections and who tend to look harder, to cite more of the detected violations, and to assess maximum penalties in firms with higher accident rates. Similarly, OSHA managers are aware of aggregate accident figures and know that they will be criticized when rates go up. As a measure of accident rates, annual safety and health incident rates were aggregated at the state level from annual Bureau of Labor Statistics survey data for the years 1974–1983.[2]

Second, we expect that enforcement should be sensitive to general economic conditions, despite the strict statutory language requiring inspectors to cite any observed violation regardless of economic conditions. In informal interviews, OSHA enforcement officials at all levels expressed concern about contributing to unemployment when times are bad and seemed inclined to give firms more leeway when the business cycle was down. They are more willing to be aggressive when business is good and employment is less a problem because firms have greater resources to respond and workers are more likely to support the efforts of OSHA inspectors. Furthermore, OSHA is less politically vulnerable to business complaints during good times than during bad times. Annual state data on unemployment and per capita income were collected to test the effect of economic conditions on enforcement actions. We expect enforcement to be inversely related to unemployment and positively related to income, although the use of income as a proxy for economic conditions is complicated because it relates to a range of other economic factors with unknown relationships to enforcement.

It is possible that task variables such as accident rates and economic conditions will affect enforcement indirectly by increasing political demands for enforcement (Moe, 1985a). Because they include political variables to control at least partially for changes in political demand, our estimates for task variables primarily reflect the direct task responsiveness of OSHA. Also, because we are concerned with changes in enforcement over time as well as between states, we recognize that even after changes in the agency's environment are detected, organizational adjustments require time to plan, implement, and institutionalize. Since only annual data was available, our model assumes a one-year lag before the full effects of political and task factors on enforcement activities are felt.

HYPOTHESIS 3. *Agencies tend to respond instrumentally to task changes and symbolically to political changes.*

Agencies can respond to political and task influences with activities that vary in their costs and their perceived likelihood of producing desired effects. Given the number of external factors affecting the industrial accident rates OSHA is charged with moderating (Mendeloff, 1979; Smith, 1976), political oversight of OSHA—as of most agencies—is based on measures of enforcement activities rather than on outcomes (e.g., accident rates). Because the relative effectiveness of different activities is not well established, particularly for those not actively involved in the process, bureaucrats may have considerable discretion in deciding which activity to increase to satisfy increased political demand. The hypothesis to be tested here is that bureaucrats will respond to political demands by changing lower-cost, "symbolic" output that may help generate the desired political support, even if it may have no effect on accidents, but will respond to task factors with "instrumental" output that agency professionals consider to be more likely to affect outcomes, even though it is also more costly. In organizational terms, responsive bureaucrats cope with the political environment by buffering critical enforcement activities and with the task environment by monitoring task changes and routinely altering critical activities to adapt to changes.

Studies assessing the effectiveness of OSHA enforcement have focused primarily on three activities for which data are readily available: inspections, citations for violations, and assessed penalties (Jones, 1984; McCaffrey, 1983; Viscusi, 1979). In these studies, the number of inspections is generally used to represent the probability of finding a violation, while the number of citations and the amount of assessed penalties reflect the extent of punishment imposed for violating the law. Together, they are assumed to determine the added incentive to comply with the safety and health standards produced by OSHA's enforcement effort. Empirical findings about the effectiveness of these activities in reducing accident rates are mixed, but the administrative nature of each activity suggests considerable differences in the instrumental value of each in reducing the risk of workplace accidents (Centaur Associates, 1985).

The number of inspections is seldom found to affect accident rates, perhaps because it can be increased simply by visiting more places for a shorter period, which increases the visibility of enforcement efforts but may or may not increase the likelihood of finding violations. The number of citations can also be changed with relatively low cost by citing relatively minor violations that exist in most workplaces because a firm is unlikely to expend resources appealing minor citations with small monetary penalties. Inspections and nonserious citations, then, provide a relatively inexpensive symbolic response that at least makes enforcement efforts more visible to OSHA's labor clients.

On the other hand, an increase in the number of citations for serious violations—those imposing imminent danger of injury—is more likely to provoke resistance from cited firms because serious citations impose higher fines, increase the likelihood of future inspections, and enhance the threat of liability if injuries do occur. For the same reasons, serious citations also impose the greatest incentive to correct the kinds of violations most likely to cause accidents and, therefore, are generally viewed by OSHA officials as an important instrumental response mechanism. Similarly, increases in serious and even nonserious penalties are more likely to impose greater costs on OSHA through increased resistance by firms but are considered effective instrumental responses to increases in accident risks.

Consequently, we expect that political responsiveness will be most pronounced for inspections and nonserious citations, while task responsiveness will be more evident for serious citations and penalties. To test this hypothesis, state-level enforcement data on inspections, citations, and penalties were collected from annual OSHA enforcement reports, with serious and nonserious citations and penalties recorded separately.[3] To control for variance due to the difference in the size of states, all figures were standardized per thousand manufacturing workers because OSHA enforcement focuses primarily on these workers.

HYPOTHESIS 4. *State agencies are more responsive than federal agencies.*

OSHA provides a unique opportunity to compare the responsiveness of state and federal bureaucracies performing the same task because authorized state agencies are allowed to enforce the federal law if OSHA certifies that state programs are at least as effective as the federal counterpart. Twenty-two states chose this option for most of the period under study. The summary data for political and task

Table 1. Comparison of State Level Political and Task Factors for State and Federal Programs, 1974–1982

Factors	Mean and Standard Deviation for State Programs $\overline{X}$	State Programs sd	Federal Program $\overline{X}$	Federal Program sd	Difference in Mean as Percent of Federal Program Means
Political Factors					
Labor (Complaints per 1000 workers)	3.06	3.9	1.81	1.2	69
Congress (Mean COPE-NAB scores)	-7.68	45	-6.52	39	-18
Governor (Democrat = 1, Republican = 0)	0.60	.50	0.71	.46	-14
State legislature (% Democrats)	63.83	16	65.10	21	-2
Task Factors					
Accidents (per 1,000 workers)	9.48	1.6	8.93	1.1	6
Income (per capita, 1972 dollars)	4,794.80	780	4,543.40	580	6
Unemployment (% workforce)	7.18	2.2	6.90	2.1	4
Standardization Factor					
Workforce in manufacturing (in 1,000s)	361.69	420	422.60	430	-14

Sources:

Governor: *Congressional Quarterly Weekly Report,* November issues for election years, 1974–1982.
State legislature: *The Book of the States,* from 1976–77 to 1982–83.
Congress: *The Almanac of American Politics,* 1978, 1980, 1982, 1984.
Accidents: *State Data on Occupational Injuries and Illnesses,* Bureau of Labor Statistics, U.S. Department of Labor, 1982 and 1983.
Unemployment, income, workforce in manufacturing: *Statistical Abstract of the United States,* Bureau of Labor Statistics, U.S. Department of Labor, 1977, 1979, 1981, 1983.

variables presented in Table 1 indicate that the labor force in state-program states is considerably more involved in OSHA enforcement, yet the states tend to be somewhat more Republican. They are smaller and slightly richer, with somewhat higher unemployment and accident rates. The differences in political and task factors are minor compared with the differences in standardized enforcement measures presented in Table 2, where states with state programs consistently score higher than states with federal programs.

Why some states chose to pay approximately 50% of the costs for the right to enforce OSHA regulations with state agencies is not clear from this data. Most analysts agree that states assume regulatory responsibility with the expectation that the state bureaucracy would be more responsive to state conditions, presumably to business interests assumed to dominate at the state level (Marvel, 1982; Williams and Matheny, 1984). However, greater enforcement action in states with state programs indicates that OSHA is effective in setting legally required minimum enforcement standards that business interests would be little in-

Table 2. Comparison of Selected State-Level Enforcement Data for State and Federal Programs, 1975–1983

	Nonserious Violations		Serious Violations		Total Penalties ($)	
Fiscal Year	State	Federal	State	Federal	State	Federal
1976	103.2	37.2	1.5	1.2	1,985.5	1,027.6
1977	88.1	20.1	2.6	2.6	1,246.2	863.8
1978	78.9	11.6	6.4	3.7	2,245.5	1,156.1
1979	68.7	8.7	6.3	3.6	2,102.1	1,204.1
1980	64.2	9.1	6.8	4.3	1,944.6	1,559.0
1981	64.8	9.2	7.0	4.3	1,980.0	1,577.6
1982	60.1	7.1	6.1	2.4	1,317.3	519.8
1983	60.4	8.4	7.2	2.8	1,160.3	686.3

Source: Data provided by the Occupational Safety and Health Administration from their computerized data base for federal states and from annual reports for state-program states.

Note: Violations and penalties are standardized for interstate variations by calculating them per 1,000 workers employed in manufacturing in the states.

terested in exceeding. Thompson and Scicchitano's (1985) analysis of the OSHA selection process is probably correct in concluding that states with the greatest geographic distance and perceived differences from Washington were most likely to opt for their own programs. In any case, there is no clear bias in the state program selection process that would preclude using observed differences to compare the responsiveness of state and federal agencies.

Structural differences between the state and federal programs support the assumption that state bureaucracies should be more responsive to political demands, particularly demands from state institutions (Kaufman, 1960; Marlow, 1981, Marvel, 1982; Scholz, 1982). State bureaucracies are small enough to be able to rely more on informal, personal supervision than can federal bureaucracies, which require more formal procedures, standards, and reports to ensure uniformity of actions in geographically dispersed areas. State bureaucracies generally have fewer levels of hierarchy between field personnel and political appointees, who are more sensitive to the political environment than civil service professionals. Even state civil servants are more likely to identify career opportunities with the state and, therefore, to be more sensitive to state political interests.

In addition, there are more elected state officials per state than elected federal representatives, and their access to state bureaucracies is likely to be greater than to the federal bureaucracy. Indeed, one of the purposes of standardized procedures and placement rotation in the federal bureaucracy is to reduce political responsiveness to the "parochial" interests of local elected officials. Furthermore, the federal program does not even have a coordinating office below the regional level as a focus for state-level political demands.

The expected difference in task responsiveness between the two programs is less clear, particularly because the difference in civil service systems and professionalism is not well known. Some argue that the characteristics that increase the political penetrability of state bureaucracies also decrease professional concerns with statutory goals (Marlow, 1981; Marvel, 1982). Professional prestige, salaries, civil service protection, training, and career opportunities are generally assumed to be better in federal than in state enforcement programs. However, complaints of legalistic, "nit-picking" enforcement by federal OSHA, particularly in its early years, indicate that the federal agency was dominated by inflexible, standardized decision routines that provided little room for professional task responsiveness (Bardach and Kagan, 1982; Smith, 1976). To test whether state and federal programs differed significantly in political and task responsiveness for our data set, a standard dummy variable analysis was used to estimate the difference in slopes and intercepts of the two different groups of states.

Method of Analysis

Annual data for the variables discussed in the previous sections and listed in Tables 1 and 2 were gathered for all states for the period 1976 to 1983, providing a total of 400 observations. The regression model estimates the impact of the lagged political and task variables on each of the enforcement variables:

$$\begin{aligned} \textit{Enforcement}_{i,t} &= C \\ &+ \textit{Political Factors}_{i,t-1} \\ &+ \textit{Task Factors}_{i,t-1}, \end{aligned} \qquad (1)$$

where i represents the states and t the fiscal year. Separate slope and intercept terms for state and federal programs were estimated by using a standard dummy variable analysis. The problems associated with analyses that pool time series from multiple state units require special estimation techniques (Stimson, 1985). Since estimations based on pooled state data are well suited to studying bureaucratic output and policy impacts, but underused, details about the specific form of the estimated equation and the Parks method of estimation are included in the Appendix. By combining cross-sectional and time-series effects, it should be noted we are making the simplifying assumption that spacial and temporal relationships are similar enough to be combined; for example, a one-unit difference in accident rates is assumed to have the same effect on enforcement whether it represents the difference between two states or between two time periods for the same state.

Table 3 presents the estimated coefficients for the six categories of enforcement activities that were studied, with each column representing an independent estimation for each activity. Results for state and federal programs are presented separately, except that separate estimates could not be calculated for variables representing presidential administrations and governors.[4] The starred estimates in bold print indicate that the factor labeled on the left had a statistically significant impact on the enforcement activity, and underscoring indicates that the estimated impacts on state and federal programs are significantly different from each other.

The results presented in Table 3 appear to be quite robust.[5] Although the following discussion of findings will focus only on statistically significant results, coefficients generally had the expected signs, even for estimates insignificantly different from 0. The unadjusted R^2 of the equations ranged from .4 to .6, indicating that about half of the variance in enforcement was accounted for despite the lack of several theoretically interesting measures expected to influence enforcement.

Discussion of Results

Agency Responsiveness to Multiple Political Signals

Two conclusions about political responsiveness stand out clearly from the analysis. First, OSHA exhibited a remarkable responsiveness to all political factors included in Table 2. Each political factor significantly affected at least two enforcement activities, and overall, 27 of the 54 possible coefficients were significant with the expected sign. The four unexpected negative signs for nonpresidential factors will be discussed below.

Second, the direct interest group measure for labor clearly exerted the most consistent influence on enforcement activities, significantly affecting every activity. The magnitude of labor's impact on enforcement during the period is more difficult to compare with other variables because the variables have no common

Table 3. Enforcement Activity Response to Political and Task Factors

		Citations		Penalties		
Factors	Inspections	Nonserious	Serious	Nonserious	Serious	Total
Political Factors						
Carter (FYs 1978–81)	1.09	**2.49***	**1.40***	**-50.17***	70.58	-49.63
Reagan (FYs 1982–83)	5.22	-2.43	.87	**-129.41***	-283.67	**-195.58***
Governor	**1.98***	**10.30***	.26	**95.42***	35.13	**116.69***
Congress						
State	.04	.01	.01	**1.06***	–.39	**1.45***
Federal	.02	**.17***	.00	**.98***	1.01	**2.18***
State legislature						
State	**.32***	**.58***	–.02	1.14	-1.92	**-1.95***
Federal	**.03***	**–.28***	–.03	-3.11	-5.46	-2.18
Labor						
State	**5.23***	**7.46***	**1.01***	**68.38***	**161.24***	**207.23***
Federal	**1.01***	**-3.62***	**.53***	**-22.35***	**124.13***	**187.36***
Task Factors						
Accidents						
State	.23	.63	–.06	**5.90***	**10.68***	**7.55***
Federal	–.00	–.28	.01	**-4.51***	**5.59***	**.62***
Unemployment						
State	–.02	.19	**–.01***	**3.90***	**-1.98***	**-3.89***
Federal	–.06	–.07	**–.01***	.42	**-1.47***	–.39
Income						
State	.00	**.03***	.00	–.00	.02	.15
Federal	–.00	**–.01***	–.00	–.15	–.19	–.06
Intercept Terms						
State program	**-42.35***	**-197.98***	6.63	-769.00	303.87	-436.82
Federal program	**34.43***	135.20	11.81	1317.13	1858.47	595.46
R^2 (uncorrected)	.64	.62	.45	.43	.50	.54

Note: The data used for this analysis are the same as those used for Table 2 and come from the same sources. The figures are estimated regression coefficients from the pooled data for all 50 states from 1976 through 1983, calculated by the Parks method for the equation presented in the Appendix. Federal coefficients are calculated to be comparable with the state coefficients, using the federal dummy variable. Where starred estimates are shown in bold print, the factor labeled on the left had a statistically significant impact on enforcement activity at the .05 level; the underscored estimates indicate that the estimated impacts on state and federal programs are significantly different from each other.

*Statistically significant at the .05 level (t test), with 383 degrees of freedom for t statistics. Underscoring indicates that the difference between the state and federal programs is statistically significant at the .05 level.

scale of measurement and because inferences about relative impacts based on regression coefficients are statistically problematic due to multicollinearity and measurement error problems (Moe, 1985, p. 1109). Since the relative impact of each variable depends on its variance during the period as well as the responsiveness coefficient, we might ask what impact each variable is estimated to have when it changes an amount equivalent to one standard deviation (reported in Table 1). For example, the impact on total penalties in the state program of an increase equivalent to the standard deviation would amount to $816 for labor complaints, compared with $65 for Congress, −$31 for state legislatures, $12 for accident rates, and −$8 for unemployment. This effect is even greater than the −$195 change representing the entire effect of the Reagan administration. Using this crude test, labor's impact on all enforcement variables is considerably greater than that of other variables, although we cannot claim from this test that the difference is of statistical significance. The consistency and magnitude of the effect of labor on all enforcement actions underscores the importance of direct enforcement resources provided by interest groups. We suspect that a measure of direct business actions would similarly be quite important in shaping enforcement actions.

Presidential Administration. The results confirm the importance of presidential influence over administrative actions (Chubb, 1985; Moe, 1982, 1985a). Both the Carter and Reagan administrations were primarily concerned with reducing OSHA's early reputation as a "nitpicking" agency that repeatedly cited insignificant violations while ignoring more serious ones. In response, OSHA significantly reduced the penalties associated with nonserious violations in the years following the Ford administration, although, surprisingly, the number of nonserious citations was not significantly reduced in response to Reagan and actually increased under Carter. The Carter administration, reflecting the traditional democratic affiliation with labor interests, shifted penalties from nonserious to more serious safety-related violations, resulting in no significant decrease in total penalties. The Reagan administration, reflecting the traditional Republic concern with business interests, reduced penalties for nonserious citations by even greater amounts without a corresponding increase in penalties for serious citations, leading to a significant decline in total penalties as well.

Although the separate effect of presidential administration on state and federal programs was not estimated because of technical difficulties, the enforcement data in Table 2 suggest that presidential administrations may be able to increase OSHA activities in state-program states (as Carter did) more readily than they can decrease them (as Reagan attempted). The drop in serious violations and in total penalties during the Reagan years (the 1982 and 1983 fiscal years) was much more dramatic for the federal program than for the state programs, while the initial rise in serious violations under Carter (fiscal year 1978) was more dramatic for state programs. This result supports the importance of the statutory provision that provides that federal OSHA sets minimum but not maximum enforcement levels for state programs.

Congress. A prolabor congressional delegation increased nonserious and total penalties in both state- and federal-program states but increased nonserious citations only in federal-program states. Given the lack of direct congressional authority over the state programs, it is surprising that only nonserious citations exhibited significantly greater responsiveness for the federal program compared with state programs (coefficients are underlined in Table 3 when differences between state and federal programs are statistically significant). The similar effect on both state and federal penalty activities suggests that the resources provided by local electoral networks are more important to agencies than formal resources controlled by Congress.

Although studies of the Federal Trade Commission (Weingast and Moran, 1983) and the National Labor Relations Board (Moe, 1985a) found that congressional committee membership had a significant effect on aggregate national enforcement actions, no significant influence was found on state-level OSHA enforcement activities. The measures of party identification of OSHA's oversight committee members (1 for each Democrat, −1 for each Republican, and 0 for states with no members) for Senate, House, and combined committee membership were insignificant in all equations and, therefore, dropped from the final estimates presented in Table 3. The relative importance of the state's delegation as a whole compared with committee membership again indicates that for OSHA, district action may be a more potent congressional mechanism for influence than formal committee oversight (but see Chubb, 1985). It should be noted that OSHA officials firmly deny any informal influence of elected officials at the local level, but the pervasive influence of local interest group actions combined with the likely influences of local electoral networks indicate the need for further studies of the complex, subtle interactions between political and bureaucratic institutions at the local level.

State Governors and Legislators. As expected, states with Democratic governors had significantly more enforcement actions in almost every category, presumably because of the governor's proximity to administrative channels, both federal and state. As noted earlier, coefficients for state and federal programs were not estimated separately, eliminating another test of the relative importance of informal networks and local resources versus formal authority. The results for the state legislators provide some evidence that both mechanisms operate, since a higher percentage of Democrats in state legislatures significantly increased inspections in federal-program states where the legislature has no formal authority but increased both inspections and nonserious citations in state-program states where they do have formal authority. Legislative influences were less pervasive and consistent than gubernatorial influences because the large number of legislators diffuses that source of influence. The unexpected negative influence on federal program nonserious citations might involve the kind of expected tradeoff discussed below for labor, but the negative influence on state program total penalties remains puzzling, perhaps reflecting the inadequacy of our simple assumptions about party coalitions in state legislatures.[6]

Labor. As noted, direct labor interaction through the filing of complaints had the most consistent effect on all enforcement output. State programs were significantly responsive for all output, and the federal program showed positive responsiveness for everything but nonserious citations and penalties. These negative relationships must be understood in conjunction with the positive relationship for serious citations and penalties because higher levels of labor complaints appear to induce a shift in resources from nonserious citations to serious ones with considerably higher penalties (thus the higher total penalties despite the decline in nonserious penalties). This tradeoff in the federal program may reflect successful attempts by management to improve the productivity of OSHA's complaint responses, which in the early 1970s were considered by many to have unduly dominated the determination of inspection targets in areas where unions were most active. In the state programs, no such tradeoff takes place, suggesting that sufficient enforcement resources are available to attend to both serious and nonserious violations.

Agency Responsiveness to Task Signals

The response to task signals was significant but less consistent than the response to political signals, with significant coefficients in the expected direction occurring in only 11 of 36 possible relationships, or about 30%, compared with 50% of the possible relationships for the political factors. On the other hand, if we limit our attention to the instrumentally most effective enforcement activities, serious citations and serious penalties, both accident and unemployment rates appear, along with labor, to be the most important influences on enforcement. Once labor is excluded, the political factors representing electoral institutions show 13 significant coefficients affecting inspection, nonserious citations, and nonserious penalties—the more symbolic responses discussed earlier—compared with only one significant coefficient (the Carter Administration) for serious citations and penalties. These findings support the argument that symbolic responses are reserved for changes in elected officials, while instrumental responses in key enforcement activities are reserved for task environment changes relevant to formal statutory objectives. The impressively robust effect of labor on all activities indicates that consistent, day to day contacts between interest groups and the enforcement bureaucracy bring the most consistent responses, both symbolic and instrumental.

Higher accident rates, the task variable most directly related to OSHA's statutory goals, brought significant increase in all penalties except for nonserious violations in the federal program, where the same tradeoff discussed previously for labor appears to decrease penalties for nonserious violations as attention is focused on serious violations. Higher unemployment, the most relevant measure of business cycle activity, led to the expected reduction in serious citations and serious penalties. Here the tradeoff between serious and nonserious penalties is evident even for the state program, where a greater number of serious violations apparently get reclassified and penalized as nonserious when higher unemployment signals an adverse business climate, leading to the negative coefficient for serious penalties and the positive coefficient for nonserious penalties. Higher per capita income affects only nonserious citations and has an unexpected negative effect in federal programs. The poor performance of this indicator probably reflects confounding relationships between per capita income and government budgets as well as industrial development and related business/union political strengths.

Responsiveness of OSHA State Agencies and the Federal Agency to Political and Task Signals

The results in Table 3 provide strong evidence that, for OSHA enforcement, state bureaucracies are considerably more responsive than the federal bureaucracy to all task and political factors, with the expected exception of Congress. Significant differences in the slope estimates for state and federal programs were found in the 15 relationships with underlined coefficients in Table 3, and the state coefficient unambiguously indicated higher responsiveness in the expected direction in all but three cases. Two of these cases involve the aforementioned tradeoff for nonserious citations and penalties related to unemployment; the state program slope is in any case greater than that of the federal program for the instrumentally important serious and total penalties. The other case relates to congressional influence, where the main surprise is that the federal agency is not significantly more responsive for all enforcement activities. The comparison of the 20 cases in which at least one program has significant estimates (indicated by an asterisk in Table 3) supports the same general conclusion because state program coefficients are greater in the expected direction in all but four comparisons, three involving Congress and unemployment as already explained and one involving the state legislature.

It is interesting to note that with all variables set to zero, the state program intercept term indicates a lower level of enforcement than is indicated for the federal program. Thus, the greater responsiveness of state agencies to political and task demands apparently produces the observed higher mean level of enforcement by state agencies indicated in Table 2. In sum, bureaucrats in both programs respond to different levels of political and task conditions, but state program bureaucrats respond with greater changes in output and with fewer constrained tradeoffs between serious and nonserious violations.

Conclusion

The image of public bureaucracy reflected in this study is that of an organization that responds rationally to political demands but does so in a complex, federalist environment in which statutory commands and oversight by central institutions provide only one set of conflicting signals. The role of federal agencies in the American policy process is not simply one of translating central political decisions into organizationally efficient routines because central institutions seldom can provide the bureaucracy with sufficient resources for such implementation. Instead, the creative role of the bureaucracy requires the development of organizationally feasible tasks that will gain and maintain sufficient support from critical actors in multiple operational arenas without undermining central support needed for formal budgets and statutory adjustments.

Thus, bureaucracy plays the important and seldom-recognized role of integrating political demands made at various levels of the American federalist system by incrementally adapting central policies to fit into varied and changing local conditions. The agency's organizational imperative to develop stable routines that provide it a secure existence in its multiple task environments force it to balance the administrative need for uniformity of tasks with the demands for diversity of services in different environments. As agencies adjust and modify routines in response to pressures from groups with formal and informal resources, the political system "learns" which policy interventions work and what they can do. Changes in central policies and oversight by central institutions provide only one input into this process, one that the agency itself helps shape through its strategic responses.

Understanding the responsiveness of bureaucracies to the range of political and task factors in their environment is an important step in understanding the role of federal agencies in the policy process. This study of OSHA indicates that even the relatively isolated enforcement routines for standardized national regulations respond consistently to daily interactions with interest groups involved in the enforcement process. Professional reactions to statutory concerns, represented in the study by accident and unemployment rates, also elicited strong instrumental responses, while responses to elected officials tended to emphasize more symbolic actions. The smaller size and greater flexibility of state bureaucracies allowed greater responsiveness than was observed in the federal bureaucracy.

These results raise three research questions of considerable importance for understanding the role of bureaucracy in democratic theory and for the design of policy institutions. First, we need to know more about the patterns of bureaucratic responsiveness. For example, do legislators and interest groups exert their influence on the state bureaucracy as a whole, or would further disaggregation of enforcement to functional categories (industries) or electoral districts indicate an even more localized pattern of response to political pressures? Do contiguous political and administrative boundaries increase political responsiveness at the cost of task responsiveness?

Second, we need to know more about specific adaptive mechanisms through which these changes in political and task conditions affect incentives, professional norms, and standard procedures in enforcement bureaucracies. Case studies of the enforcement process have not analyzed the kinds of specific adaptive mechanisms that would explain the results of this analysis, perhaps because personalities, idiosyncrasies, and the standard procedures that dominate the daily world of enforcement agencies tend to overwhelm the systematic political influences that appear more clearly in quantitative analyses such as this one. Furthermore, the pejorative connotation of patronage, the illegality of political influence on individual enforcement procedures, and the positive value of professional, autonomous enforcers all lead bureaucrats to respond defensively to questions of political influence, creating research problems similar to those encountered in trying to analyze the mechanisms by which lobbyists influence legislators (Manley, 1970). More focused studies are needed to isolate and catalogue the complex and subtle mechanisms that link political and bureaucratic institutions on the local and central levels.

Finally, analyses of central political controls over bureaucracy need to take account of the integrative function of agencies in our federal system. At least part of the difficulty between agencies and central institutions represents the ongoing conflict between central and local political leaders over incremental adjustments of government activities to changing conditions in different communities. Just as changes in electoral laws alter the responsiveness of elected officials to different groups, so do changes in the organization, budgets, and central supervision of agencies alter their responsiveness to different elected officials and interest groups. The great advantage and the major difficulty in the development of a normative theory of bureaucratic enfrancisement is that it must simultaneously consider the problems of democratic representation in a federalist system and the pragmatic requirements of adaptive, productive service delivery organizations.

Consider, for example, the problem of designing regulatory policies such as OSHA. Responsive enforcement of uniform federal rules provides one potentially useful compromise between the inevitable inefficiencies associated with imposing uniform rules on state units differing in their policy demands and task conditions (Advisory Commission, 1984; Bardach and Kagan, 1982) and the "balkanization" problems and excessive technical costs associated with state-level regulation (Scholz, 1982). The appropriate level of responsiveness, however, must certainly depend on the implementation resources of contending groups in different arenas as well as on the kinds of costs associated with excess uniformity or variability for the particular policy issue. Furthermore, appropriate responsiveness mechanisms must balance the need to control bureaucratic self-interest, to promote pragmatic compromises among competing groups, and to minimize disruptive alterations in productive routines.

The answers to these critical problems require an integrated perspective concerned with both political institutions and public bureaucracies. Formal models analyzing political-bureaucratic interaction at the central level (Bendor, et al, 1985; McCubbins, 1985; Miller and Moe, 1984) may provide a useful starting point for this development. However, analyses of agency accountability to national institutions need to include questions about agency adaptability to multiple legitimate pressures in our federalist system. The rich redundancy of political demands in the federal system and the flexibility of bureaucratic responses, even when enforcing standardized laws, provides an as yet poorly understood component of our disjointed incremental political system.

Appendix

There are several models for estimating regression equations that are appropriate for different kinds of pooled time-series and cross-sectional data sets (Stimson, 1985). Because state-level aggregate data are taken from contiguous geographic units (states) defined by arbitrarily drawn boundaries, both heteroscedasticity and mutual correlation problems between cross-sectional observations are likely to exist (Berk, Hoffman, Maki, Rauma, and Wong, 1979, p. 402; Kmenta, 1971, p. 512). For the serial observations, we suspect that stochastic forces affecting individual state enforcement in any given time period affect the enforcement in proximate time periods as well. However, because most longitudinal regulatory studies suggest that with yearly data, higher order autocorrelations decline rapidly and tend to be negligible (e.g., Berk et al., 1979; Moe, 1985a) and because our data consist of a short time series of eight annual observations, we believe that a first-order autoregressive assumption for the data is appropriate. Combining these assumptions, we require an estimation model that allows heteroscedasticity and mutual correlation to exist between cross-sectional units and first-order autoregression to exist within the individual unit.

Parks (1967) has developed a consistent and asymptotically efficient three-step estimation technique for such a model that is contained in the commonly available SAS statistical package. Very briefly, the first step of the Parks estimation method uses ordinary least squares (OLS) to estimate the parameters of the autoregressive model; the second step applies OLS to the transformed model (in which autocorrelations have been corrected) to estimate the contemporaneous (mutually correlated) covariances; and the final step uses the estimated covariances to get the regression coefficients. The Parks method takes account of three possible error terms: (1) that associated with a first-order, autoregressive error structure; (2) that associated with heteroscedasticity; and (3) that associated with mutual correlation between cross sections. Therefore, it is expected to produce more efficient regression estimates that an OLS method.

In order to examine simultaneously the effect of both state and federal programs on OSHA enforcement, we employ the dummy-variable technique to test for differences in both the initial level of enforcement activity and bureaucratic responsiveness between the two groups within one overall model. The test for the program differences will be carried out by testing for intercept dummy and slope dummy variables representing the differential effects between the two programs. The exact model to be tested is therefore

$$\begin{aligned} E_{i,t} = c + b_0 F + b_{1h} p_{ih,t-1} + b_{2k} T_{ik,t-1} \\ + b_{3h} F^* P_{ih,t-1} + b_{4k} F^* T_{ik,t-1} \\ + u_{i,t}, \end{aligned} \qquad (2)$$

where

$$i = 1,2, \ldots, N;$$
$$t = 1,2, \ldots, T;$$

$E_{i,t}$ represents the individual enforcement measure for state i during period t;

F is the intercept dummy variable that equals 1 for federal program states and 0 for state program states;

$P_{ih,t-1}$ is the lagged measure of political variable h for state i;

$T_{ik,t-1}$ is the lagged measure of task variable k for state i;

$F^*P_{ih,t-1}$ and $F^*T_{ik,t-1}$ represent interactive variables of federal programs and groups of lagged political and task variables;

c and the subscripted b terms are the regression coefficients to be estimated;

and

$u_{i,t}$ is the stochastic disturbance that is assumed in the Parks model to have the following properties:

$$E(u_{it}^2)\ s_{ii} \qquad \text{(heteroscedasticity)},$$
$$E(u_{it}u_{jt}) = s_{ij} \qquad \text{(mutual correlation)},$$
$$u_{it} = \varrho_i u_{i,t-1} + e_{it} \qquad \text{(autoregression)},$$

where

$$e_{it} \sim N(0, V_{ii}),$$
$$E(u_{i,t-1}e_{jt}) = 0,$$
$$E(e_{it}e_{jt}) = V_{ij},$$
$$E(e_{it}e_{js}) = 0 \qquad (t \neq s),$$

and

$$i,j = 1,2, \ldots, N.$$

Note that in this model the value of the parameter ρ (first-order autocorrelation) is allowed to vary from one cross-sectional unit (state) to another and that the variances of e are denoted by the symbol V. Also, the initial value of u is assumed to have the following properties:

$$u_{i0} \sim N(0, V_{ii}/1 - \rho_i^2),$$
$$E(u_{i0}u_{j0}) = V_{ij}/1 - \rho_i\rho_j.$$

Further information can be found in Kmenta (1971, pp. 513–14).

In the model, the constant c represents the initial level of state program enforcement. The estimated coefficient of intercept dummy variable F, b_0 indicates the difference in the initial enforcement level between federal and state programs. The estimates of b_{1h} and b_{2k} measure the effects of individual political and task variables on enforcement activities of state programs. Finally, the estimated coefficients of interactive variables—b_{3h} and b_{4k}—measure the direction and significance of the difference in enforcement responsiveness between the federal and state program. The estimates of responsiveness for the federal program, as presented in Table 3, are then the sum of b_{1h} and b_{3h} for political variable h and of b_{2k} and b_{4k} for task variable k.

Notes

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1. We recognize that the number of complaints provides only a crude indicator of the complex range of factors influencing enforcement interactions among business, labor, and OSHA, particularly because labor at times uses complaints to harass business during contract negotiations and because OSHA responds only minimally to such negotiation-related complaints. Since the direction of bias from these influences should make it more difficult to find significant instances of labor influences, they should not jeopardize conclusions when significant relationships are found.

We also attempted to test whether bureaucrats were sensitive to the relative electoral strength of political groups or only to electoral pressures as filtered by elected representatives but found no satisfactory measure of business and labor electoral strength. Campaign contributions, the percentage of the labor force unionized, and the percentage of income produced in manufacturing and construction (the two business classifications most affected by OSHA) were considered but rejected, the first two because of missing data and the last because it would not distinguish business from union strength in the state.

2. The survey did not include six states for the entire period and seven other states for the latest years. Estimates for states not in the survey were made by multiplying the percentage of total state workers in each two-digit Standard Industrial Classification (SIC) category by the national accident rates for those categories and summing over all categories. The effect of replacing state data with national estimates should decrease variance among states and therefore diminish the likelihood that significant responsiveness would be found.

3. Data for states in the federal program were obtained from a computer printout of annual data prepared by OSHA at the authors' request, and data from states with approved state programs were obtained from annual state report forms supplied by OSHA's Office of State Programs. Although the lack of reporting consistency among states for many of the items on these forms precluded the analysis of other interesting variables, the data on inspections, citations, and penalties are generally accepted as relatively consistent.

4. Attempts to estimate separate state and federal coefficients for the administration and governor variables were unsuccessful because of the instability of estimates when dummy slope variables interacted with these dummy variables, indicating that the large number of results of 0 and 1 introduced multicollinearity problems.

5. A number of analyses were performed that are not reported because of space limitations. Changing the variables in an equation had some effect on the magnitudes of different estimates but did not result in the reversal of signs and had only minor effects on determining which variables had statistically significant effects. We also determined, on the basis of standard f tests, that the sum of residual squares in the pooled model was not significantly greater than the combined sum of the residual squares of individual cross-sectional analyses, thus providing a statistical justification for preferring the pooled estimates over separate cross-sectional estimates of coefficients.

6. We attempted to account for the obvious difference between southern Democrats and others by using a dummy variable to represent the southern states, but the interaction term was dropped because it was insignificant in all equations and did not eliminate the unexpected sign.

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THE POLITICS AND ECONOMICS OF THE NEW CITY DEBT

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This paper contrasts nonguaranteed city debt with taxation and general obligation debt. Drawing upon Bureau of the Census, Advisory Commission on Intergovernmental Relations (ACIR) and Municipal Year Book *data, the analysis shows that both per capita tax revenue and outstanding general obligation debt are best explained by background factors such as total population, functional scope, and region, while fiscal strain plays a secondary role. Political structures, in the shape of legal constraints on taxation and debt and of form of government, do not account for patterns of these traditional revenue sources. By contrast, nonguaranteed debt is best explained by a model in which fiscal strain has a paramount role, but with both legal constraints on taxing and regional differentiation contributing significantly to the explanation, at least for data collected prior to 1978. The findings suggest a two-tiered model of the revenue side of fiscal decision making, with historical accommodations to powerful economic and demographic factors dominating taxing and general obligation debt, while nonguaranteed debt serves as a flexible instrument of shorter-term strategy.*

In the nearly thirty years since Herson's (1957) characterization of the "lost world of municipal government," scholars have left virtually no stone unturned in efforts to bring the methods of empirical social science to bear upon an understanding of urban politics and policy. But there are some stones left remarkably undisturbed. Even in the wake of the fiscal convulsions of the mid-1970s, research on municipal indebtedness has been quite exceptional. In part, this is because municipal finances generally occupy only a portion of the attention of urban scholars. Even within the local government finance realm, there is a bias toward analysis of expenditures rather than revenues because the former are more readily conceptualized as policy outputs (MacManus, 1977, p. 266). To the extent that revenues are of interest, the politics of taxation is usually at issue. Attention to debt takes a back seat at best.[1]

If city debt is still a part of the "lost world" of municipal politics, there is ample reason to rescue it from such a backwater. Indebtedness, like taxation or expenditure policy, is an important component of urban fiscal policy and, according to Lamb and Rappaport (1980, p. 3), an area of heavy activity:

> The most dynamic area of the investment industry today, as for the past decade, remains largely untouched in academic or trade literature: the market for municipal securities, which help to finance state and local government functions through the issuance of debt. . . . Taken altogether, the dollar volume of debt issued from 1970 to 1978 by municipalities was about double the total issuance of all corporate debt.

More important, there has been a quiet revolution in the past fifteen years with respect to city indebtedness. Long-term

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municipal debt can be divided into two types: (1) general obligation bonds, sometimes referred to as full faith and credit debt, and (2) nonguaranteed debt, encompassing a broad variety of revenue bonds.[2] Newcomer, Trent, and Flores-Kelly (1983, p. 218) aptly describe the differences between the two types:

> General obligation debt is secured by the full faith and credit of a municipality, that is, an unlimited claim on its taxes. Often this type of debt, because it is secured, carries a lower interest rate than most bond yields. . . . There are various types of nonguaranteed bonds, but they are typically sold with the expectation that repayment will be made through a specific revenue source such as a user charge.

Another important difference between the two types of debt has to do with the number of limitations and accountability devices that surround general obligation debt, as compared with revenue bonds. A large number of states set limits on the amount of general obligation debt that municipalities can have, and in many places there are also requirements for voter approval before general obligation bonds can be issued; by contrast, nonguaranteed debt is by and large free from caps and referenda requirements (Newcomer et al. 1983, pp. 218–19; Sbragia, 1983, pp. 72-73).

The quiet revolution referred to above has to do with the explosive growth in the nonguaranteed type of debt, leading to a turnabout in the mixture of debts characterizing urban government. Traditionally, general obligation debt predominated, constituting 60% of long-term municipal debt outstanding in the latter part of the 1960s and through much of the 1970s; however, by the latter part of the 1970s, the revenue bond revolution had begun, and by 1981 and 1982, nonguaranteed debt constituted the major part (59%) of outstanding municipal debt (Bureau of the Census, 1984, p. x).

Yet another reason for scholarly interest to turn to municipal indebtedness is that the growth in local government debt, and especially in the nonguaranteed type, has not occurred without some controversy, at least within the narrow confines of the intergovernmental policy community with a stake in the matter. Nonguaranteed debt has always been undertaken primarily for facilities and infrastructure development, from the earliest uses for canal and turnpike development to current revenue bonds for "sewers, gas service, telephone systems, parking, airports, ports, rapid transit, stadiums and other recreation facilities, solid waste disposal systems, college dormitories" (Lamb and Rappaport, 1980, p. 14), and the like. Criticism has arisen, however, as revenue bonds, whether for facility development or other functions, have been issued for private users:

> The controversy has been triggered by an explosive growth in the volume of tax-exempt bonds issued to benefit private users—bonds ranging from mortgages for purchases of single-family homes, to loans for corporations building pollution control facilities, and to loans to fund facilities for industrial development. (ACIR, 1984b, p. 115)

In a period in which competition for economic development at the local level makes for a blurring of the line between public and private uses, this criticism of private uses of nonguaranteed debt may seem misplaced. However, private uses exacerbate the explosive growth in nonguaranteed debt and lead to an erosion of federal revenue because of the tax exemption of interest on state and local bond issues. Not surprisingly, there has been an ongoing debate about uses of tax-exempt bonds and periodic federal efforts to limit them (Sbragia, 1983). These federal efforts include the Revenue and Expenditure Control Act of 1968, which set some limits on the volume of Industrial Development Bonds (IDBs); the Tax Equity and Fiscal Responsibility Act of 1982, which held that small issue IDBs would not be federally tax-exempt if used for various recreational or commercial purposes; and the Mortgage Subsidy Bond Tax Act of 1980, which restricted the tax-exempt status of revenue bonds issued to subsidize single family housing mortgages (ACIR, 1984b, pp. 118–19). Currently, the possibility of removing tax-exempt status from municipal revenue bonds is again on the congressional agenda. In short, municipal debt, and perhaps especially the nonguaranteed type, is of interest because it is problematic from the intergovernmental perspective and is chronically the focal point of federal control efforts.

Finally, the study of city debt provides an opportunity for a fresh look at the debate over *political* versus *environmental* explanations in urban scholarship—a debate that has never been adequately resolved (Peterson, 1981, p. 10). Here, political structure variables, fiscal strain measures, and background factors that reflect longer-term, exogenous forces are incorporated into a comparative analysis of urban debt and tax revenue patterns.

Hypotheses and Measurement Issues

Hidden behind the national trends in urban indebtedness is substantial variation across cities in patterns of general obligation and nonguaranteed debt. For the sample of 250 cities upon which this research is based, general obligation debt outstanding in fiscal year 1977 ranged from less than $1 per capita in communities such as Lakewood, California; Ferndale, Michigan; and Falls Township, Pennsylvania to more than $1,000 per capita in communities such as Wilmington, Delaware; Braintree Township and Taunton, Massachusetts; and New York City. A similar range of variation existed in 1981.[3] With respect to nonguaranteed debt, variation across communities is more striking. In fiscal 1977, nonguaranteed debt outstanding ranged from less than $1 per capita in many communities to more than $1,087 in Decatur, Alabama; $1,995 in Gainesville, Florida; and $2,492 in Ashland, Kentucky. In fiscal 1981, again there are many communities with less than $1 per capita in outstanding nonguaranteed debt; but there is also Jonesboro, Arkansas at $3,342 per capita and Orlando, Florida at $3,991, while Ashland, Kentucky had increased its outstanding nonguaranteed debt to $6,419 per capita.[4]

How might we account for this variation in debt across local governments? Essential to this analysis is the central theme that, partly because it is an innovative fiscal instrument and partly because it is less visible and less enshrined in accountability devices, the incidence of nonguaranteed debt will offer some important contrasts with taxation and traditional, general obligation debt. Hence, the analysis entails a comparison of the predictors on the one hand of per capita tax revenue and outstanding general obligation debt and on the other of outstanding nonguaranteed debt per capita.

Three major classes of independent variables follow. First, is a set of background variables that, largely on the basis of expenditure studies, have been deemed to be important in the analysis of urban fiscal outcomes. Just as with per capita spending, cities' per capita taxation, general obligation indebtedness, and revenue bond indebtedness might be expected to be a positive function of city size, so total *population* is considered as a predictor. Liebert (1976) makes a convincing argument that understanding urban fiscal activity requires attention to differences across U.S. cities in the scope of the functions handled by the city. For this reason, *functional scope* must be included in the analysis. The functional scope index used here is an additive one, based on a count of the following functions for which the Census of Governments reported expenditures in excess of one dollar per capita

Table 1. Regional Variation in City Taxation and Debt

Variables	Regional Means for Sample Cities			
	South	West	Northeast	Midwest
Per capita tax revenue	$154	130	302	121
Outstanding general obligation debt per capita	258	79	276	178
Outstanding nonguaranteed debt per capita	437	144	14	147
Number of cases	71	49	74	56

for the year at issue: welfare, health and hospitals, education, sewers and sanitation, highways, parks and recreation, and housing and urban renewal (or community development).[5]

Consistent with the basic theme that nonguaranteed debt is an innovative, less visible and less traditional revenue instrument than taxation or general obligation debt, we hypothesize that background variables such as functional scope and city size will be powerful predictors of per capita tax revenue and general obligation debt but quite weak predictors of outstanding nonguaranteed debt. Stated another way, taxation and traditional debt are expected to be responsive to basic, Wagner's Law-style forces (Lowery and Berry, 1983, pp. 667–68), which largely reflect a city's longer-term history. Decision making about revenue bonds, however, is more a reflection of current pressures and opportunities.

Regional variation must be taken into account for similar reasons and also because sunbelt versus frostbelt comparisons have become rather standard fare in analyses of a variety of urban phenomena. As Table 1 shows, however, regional differences in the fiscal phenomena at issue here are more complex than a simple sunbelt versus snowbelt breakdown.[6] There is some regional variation in per capita city taxes, although the difference is primarily one that contrasts northeastern cities with all others. General obligation debt is relatively high in both the Northeast and the South, but perhaps the most distinctive regional manifestation with respect to general obligation debt is the low level on average for western cities. Finally, southern cities show dramatically higher levels of revenue bond debt outstanding than any other region, while northeastern cities on average show very little involvement in this form of indebtedness.

Some of this regional variation may simply be a reflection of regional patterning in several of the independent variables outlined below. In this vein, Lineberry and Fowler (1967, p. 506) have argued that region is an interesting variable only insofar as it is an amalgam of differences in history, government structure, demographic patterns, and the like; hence a region variable constitutes a catchall explanation that fails to distinguish the relative importance of the individual variables that comprise it; analysis focusing on first-order variables that are encompassed by the region variable would be preferable. While acknowledging Lineberry and Fowler's point, this analysis treats region like the other two background variables—population size and functional scope. All three are considered, at least initially, along with the explanatory factors outlined below, such that the relative importance of the latter can be ascertained with these background factors taken into account.

Fiscal Strain versus Political Constraint and Response

Although the background variables outlined above must be considered, they do not occupy center stage on grounds of theoretical importance. That place is reserved for a comparison of the explanatory importance of the two other classes of independent variables. One class of independent variables taps various aspects of *fiscal strain;* the other reflects the *political structures* that present constraints (and opportunities) for decision makers.

Fiscal strain. Clark and Ferguson (1983, pp. 43–45) have identified three conceptions of fiscal strain:

> One is that a city suffers fiscal strain if it has a weak or declining economic base, especially characterized by population loss. . . . A second conception stresses the municipal bond market which summarizes judgment of informed investors. . . . We consider neither adequate but propose a third incorporating them as components. . . . Our conception . . . leads us to focus on how it obtains resources from its "environment." . . . As the socioeconomic base changes, municipal officials must formulate new policies to maintain balance between resources and expenditures. Cities that do not adapt rapidly enough suffer fiscal strain.

For analysis of urban tax and debt patterns in 1977, three measures, corresponding to each of the three conceptualizations of fiscal strain identified above, can readily be obtained. First, as Clark and Ferguson note (1983, p. 43), *population change* is usually the indicator of fiscal strain in the sense of a declining economic base, and data on population change from 1970 to 1975 reasonably reflect change immediately preceding the 1977 fiscal year. Second, fiscal strain, as reflected in the municipal bond market, is measured by the city's *bond rating* (Moody's) for 1977. Third, as an indicator of the failure-to-adapt conceptualization of fiscal strain, a *fiscal strain index* has been constructed, along lines suggested by Clark and Ferguson (1983, pp. 54–55). Specifically, the index of fiscal strain shows the extent of local public sector spending relative to city wealth.[7] City wealth itself is an index, comprised of the summation of two elements: (1) proportion of the city's own source revenues from the property tax times median value of owner-occupied housing and (2) proportion of the city's own source revenues from non-property tax sources times median income.[8]

These three aspects of fiscal strain might be hypothesized to relate to tax, general obligation debt, and nonguaranteed debt patterns in somewhat different ways. For example, fiscal strain in its second (bond rating) sense (so coded that poorer bond ratings have higher values) would be expected to show a positive association with levels of both per capita taxation and traditional general obligation debt. This is because, while bond ratings have often been criticized for poor predictive power, they do represent summary judgments on the part of the financial community about the current fiscal health of the community. A heavy tax burden and substantial outstanding indebtedness (of the general obligation type) are precisely the sorts of phenomena that bond-rating services count toward a poorer bond rating. On the other hand, level of outstanding nonguaranteed debt should not be hypothesized to show such a linkage with bond ratings because the latter, at least as of 1977, represent judgments about the riskiness of the city's general obligation bonds, not its revenue bonds.

Fiscal strain is also reflected in population change. However, it is important to note that either dramatic loss or dramatic gain in population may represent fiscal strain for the community—hence, percentage change in population is transformed into a squared term for the analysis. For example, dramatic population growth might lead to a high level of borrowing in order to finance new infrastructure, while dramatic population loss may mean that shrinking population bases are carrying the load of infrastructure maintenance and reconstruction, or it may stimulate city officials to undertake capital improvements in the name of economic development. In either case, it may be hypothesized that borrowing in one form or the other will escalate. On the other hand, we would not expect the same connection between population change and taxation patterns. Among growing cities, the tax base expands along with population (although not one for one); therefore, there is not necessarily an increase in per capita taxation even in the face of fiscal strains from growth. In declining cities, limits on public tolerance of taxes or public officials' attempts to regain a competitive position by minimizing taxes can act as stoppers on growth in per capita tax revenues. Intergovernmental revenues can also complicate any clean connection between urban decline and per capita tax revenues.

The fiscal strain index is hypothesized to have a positive, linear association with each of the fiscal outcomes of interest here; that is, a city that is strained by overall public expenditures that are large relative to the city's level of private wealth must presumably make heavy use of as many revenue instruments as possible to sustain its expenditure policies. Additionally, it can be hypothesized that the fiscal strain index should be a more powerful predictor of revenue policies than is population change because while the latter taps only the economic-demographic forces impinging upon the city, the former taps the mesh between city expenditure policies and these external forces. Stated another way, the relative strength of the population change variable and the fiscal strain index in accounting for tax and debt patterns provides an indirect test of Clark and Ferguson's contention (1983) that demographic and economic trends need not spell fiscal strain, if cities adapt their fiscal policies accordingly. If that contention is true, then population change per se is a relatively poor indicator of fiscal strain and presumably, then, a poorer predictor of tax and debt patterns than is the fiscal strain index.

Political Structures as Constraints. Cities' adaptive responses (or failures to respond adaptively) do not occur in a vacuum, however. While the fiscal strain measures treated above point us toward environmental variables reflecting key external forces like demographic shifts and economic trends, political structures must also be considered. Two political structure explanations, each representing a form of constraint on public officials' tax and debt decision making, are considered here.

First is a family of hypotheses stipulating that reformed cities should show different taxing and debt patterns than unreformed cities. Specifically, a key hypothesis is that communities with reformed political structures should have greater levels of outstanding nonguaranteed debt than communities with unreformed political structures; by contrast, unreformed cities are hypothesized to have higher per capita tax revenues and higher traditional general obligation debt levels than reformed cities.

The expectation of higher taxation and higher traditional debt levels in unreformed cities is consistent with previous research showing that unreformed cities are more responsive to increased spending demands than are reformed cities (Lineberry and Fowler, 1967; Lyons, 1978). Other things being equal then, unreformed cities will end up taxing and borrowing more to sustain spending for services while reformed cities are more sensitive to demands for a low tax profile.

The picture changes considerably, however, when nonguaranteed, revenue bond debt is at issue, largely because of the purposes for which this form of debt is used and because nonguaranteed debt need not interfere with a low tax profile (i.e., it is to be paid off with user fees rather than taxes). Reform governments have been supposed to result from and reflect the values of a "public-regarding ethos" (Banfield and Wilson, 1963). This ethos was supposed to be linked with programmatic commitments like urban renewal (Wolfinger and Field, 1966, p. 307) that in more recent literature (e.g., Peterson, 1981) would be categorized as "developmental."

There is considerable evidence that reformism in municipal government was historically instigated by business groups and their allies, that it institutionalized business attitudes and practices within city government, and that these reformers were motivated by larger policy interests that fit nicely into the economic development category (Hays, 1984, pp. 56–59). More specifically:

> The increasing size and scope of industry, the effect of floods on many business concerns, the need to promote traffic flows to and from work for both blue-collar and managerial employees—all contributed to this larger interest. The geographically larger private perspectives of upper-class, professional, and business groups gave rise to a geographically larger public perspective. (Hays, 1984, p. 59)

Yet, surprisingly little contemporary empirical analysis has been devoted to the alleged linkages between reformism, business dominance, and interests in citywide growth and development (for an exception, see Morlock, 1974).

Despite this relative paucity of empirical research on the matter, tacit acceptance of Banfield and Wilson's basic contention (1963) that business interests and reform government go arm in arm is quite widespread. What is most important here is that the special amalgam of business influence, growth interests, and economic development politics that is presumed to be especially characteristic of reform cities is also the condition in which revenue bonds become prominent. They are typically issued either for infrastructure improvements necessary to sustain a competitive business and economic environment (water and sewer, power, or transportation facilities) or for designated "industrial development" purposes (ACIR, 1984b, p. 117).

In short, because of the economic development uses to which it is typically directed and because it can be used expansively without harming the city's low-tax image, nonguaranteed debt is expected to be the only one of the three fiscal instruments examined here that is used more extensively by reformed than nonreformed cities. The analysis thus provides one specific test of the presumed connection between reformism and business and industry-oriented policies.

Some empirical evidence bearing on this specific version of the reformism hypothesis has already been reported. Newcomer, Trent, and Flores-Kelly (1983, p. 223) show that, on average, the dollar volume of outstanding general obligation debt in mayor-council cities exceeds the volume of nonguaranteed debt while, in council-manager cities, outstanding nonguaranteed debt predominates over general obligation debt. The limitations of this evidence are that volumes of debt oustanding are not adjusted for city population size, that a multivariate approach is lacking, and that only one element of reformism is considered. These limitations can, however, be overcome.

Level of reformism is measured here with a three-point additive index constructed from the components that are classically the focus of reformism research: *form of government, constituency representation* in the local legislature, and *partisanship* (Lineberry and Fowler, 1967; Wolfinger and Field, 1966). The council-manager form of government is counted as a reform structure, as is the absence of parties on the ballot. The distinction between at-large and district elections has traditionally been the basis for reform versus nonreform categorization on the constituency representation dimension. However, the proliferation of mixed electoral systems makes a simple differentiation on this basis more difficult. In 1978, for example, nearly 40% of the sample cities for which the *Municipal Yearbook* provides information are listed as having a mixture of council members, nominated or elected both at large and by wards. Unfortunately, information is not provided about the number of council members chosen by each method. Information on the overall size of the council is available, however, and was used to construct a constituency representation component for the reformism index. Because the essence of the at-large versus the ward distinction is whether the council member has a relatively small, localistic constituency or is freed from the particularistic interests of the neighborhood, the population per council member was computed. Cities having larger than average "constituencies" on this measure (i.e., greater than 14,000 people per council member) received an added point on the reformism index; those with smaller than average constituencies did not. On the resulting three-point reformism index, 31 sample cities (16.4%) scored zero, 40 (21%) scored one, 87 cities (46%) had two reform structures, and 31 cities (16%) had all three.[9]

In addition to this form of government index, a pair of additional variables of the political structures type were considered as predictors of tax and debt levels—one reflecting constitutional or statutory *constraints on the property tax* and one summarizing legal *constraints on general obligation borrowing* as of 1977. A host of state constitutional or statutory restrictions surrounded municipal taxing powers and general obligation indebtedness levels in many states even prior to the tax revolt era associated with the passage of Proposition 13 in 1978. Indeed, some commentators have argued that the growing popularity of nonguaranteed debt lies partly in that it is virtually free of such statutory and constitutional constraints and accountability devices (Sbragia, 1983, p. 72; Newcomer, et al., 1983, p. 219; MacManus, 1981, p. 78). The constraints prior to 1978 may have had fewer teeth than many of those that were to come later; but, to the extent that taxing and general obligation debt limitations were effective in limiting those sources of revenue, city officials might have turned more and more to revenue bonds in order to finance projects that would otherwise have been handled with general obligation bonds backed by tax revenues. In short, we may hypothesize that the stringency of legal constraints on taxing and general obligation borrowing will be negatively associated with per capita levels of tax revenue and outstanding general obligation debt (i.e., an intended impacts hypothesis) and positively associated with per capita levels of nonguaranteed, revenue bond indebtedness (a presumably unintended impact).

Some evidence of these effects is already available. MacManus (1981), for example, found that dependence on general obligation debt decreases and dependence on nonguaranteed debt increases as state imposed borrowing constraints on the former become more restrictive. She also found (1981, pp. 80–81) that reliance on nonguaranteed debt relative to general obligation debt increases sharply as property tax constraints become more severe.

While MacManus's results are consistent with hypotheses to be pursued here, the dependence measures she employs (proportions of total debt of each type) are less than optimal. A given city may show a high proportion of outstanding debt in the nonguaranteed category by

Table 2. Hypothesized Predictors of Per Capita Tax Revenue, Guaranteed Debt, and Nonguaranteed Debt

Predictor Variables	Tax Revenue	Guaranteed Debt	Nonguaranteed Debt
Fiscal strain			
Bond rating[a]	+	+	0
Squared population change	0	+	+
Fiscal strain index	++	++	++
Political structures			
Reformism index	−	−	+
Tax constraint index	−	−	+
General obligation debt constraint index	−	−	+
Background factors			
Total population	++	++	0
Functional scope index	++	++	0
Region	Northeast	West	South or Northeast

Note: Symbols reported are + for hypothesized positive relationships, − for hypothesized negative relationships, and 0 where no association is hypothesized. A duplicated symbol, as ++, denotes a relationship that is expected to be especially strong. Given the distinctive regional patterns reported in Table 1, different regional dummy variables are relevant to each dependent variable because the region showed either distinctly high or distinctly low on that variable.

[a] Poorer bond ratings are coded as having higher numerical values than stronger bond ratings to reflect the assumption that poorer bond ratings indicate higher levels of fiscal strain.

comparison with another city yet have a very low volume of outstanding nonguaranteed debt for its size—that is, it may simply have relatively little outstanding debt of either kind. The dependent variables used here are all based on per capita volumes to avoid this problem. However, the indexes of state property tax and borrowing constraint suggested by MacManus (1981) are adopted here, with some slight modification to update the 1973–74 information in her property tax constraint index with 1976–77 information.[10]

By way of summary, Table 2 shows a matrix of the three dependent variables to be compared, the independent variables to be considered, and the hypothesized relationships. The table highlights a few key comparisons emerging from the detail of the hypotheses: (1) The fiscal strain index is hypothesized to be the most potent of the fiscal strain measures and the only one that has an impact on taxation and both forms of debt in the same manner; (2) each of the political structure variables is hypothesized to differentiate between the traditional revenue instruments (taxing and general obligation debt) on the one hand and nonguaranteed debt on the other, with reformism expected to have the same pattern of impacts as tax and borrowing restrictions; and (3) background variables like population size and functional scope are expected to be most of the story in explaining tax and general obligation debt variation, but not nonguaranteed debt.

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Table 3 shows the zero order correlations between each of the dependent variables and the predictor variables.

Table 3. Correlations of Predictors of Per Capita Tax Revenue, Guaranteed Debt, and Nonguaranteed Debt

Predictor Variables	Per Capita Levels of Tax Revenue	Per Capita Levels of Guaranteed Debt	Per Capita Levels of Nonguaranteed Debt	Number of Cases
Fiscal strain				
Bond rating	.08	.07	.11	164
Squared population change	.02	−.04	.26[a]	189
Fiscal strain index	.35[a]	.38[a]	.49[a]	163
Political structures				
Reformism index	−.16	−.11	.21[b]	189
Tax constraint index	−.44[a]	−.29[a]	.23[a]	188
General obligation debt constraint	−.17	−.09	.06	188
Background factors				
Total population	.34[a]	.39[a]	−.03	189
Functional scope index	.62[a]	.49[a]	−.06	189

[a]Significant at the .001 level or less

[b]Significant at the .01 level or less

Most of the hypotheses receive preliminary support, most notably those specifying (1) that the fiscal strain index will emerge as the most potent of the fiscal strain indicators and (2) that there will be a strong role for the background variables in predicting taxing and general obligation debt but not nonguaranteed debt. In addition, reformism is related to nonguaranteed debt as hypothesized but shows no significant association with the other two fiscal outcomes. The property tax constraint index performs precisely as hypothesized, suggesting that where constraints are stringent, taxes and general obligation debt are minimized but revenue bond usage escalates. The borrowing constraint index, however, shows no significant associations.

To assess the relative importance of each of the variables, however, multivariate analysis is in order. Table 4 presents the results of a pair of multiple regression analyses—one with tax revenues per capita and the other with per capita general obligation debt outstanding as the dependent variable. In each case, dummy regional variables appropriate for the dependent variable (see Table 1) have been entered, along with predictor variables that emerged in Table 3 as promising explanatory factors.

Table 4 shows that, as expected, considerable variation in both of these traditional revenue policies can be accounted for by the same explanatory model. It is a model in which background variables such as total population size, functional scope, and region predominate. These are the types of variables that represent a city's longer-term history and which are relatively distant from immediate policy choices. Although much less important, the city's adaptive response to shorter-term environmental trends, as reflected in the fiscal strain index, is also a significant predictor because cities that bring their expenditures more into line with available wealth end up with lower per capita demands on the revenue side for either of these traditional fiscal instruments. In this multivariate analysis, the tax-constraint index fails to achieve statistical significance. In these years prior to passage

Table 4. Explaining Patterns of Per Capita Tax Revenue and General Obligation Debt, 1977

Predictor Variables	Standardized Regression Coefficients	
	Per Capita Tax Revenue	Per Capita General Obligation Debt
1975 population	.29[a]	.33[a]
Functional scope index	.29[a]	.23[b]
Northeast	.34[a]	—
West	—	-.21[b]
Fiscal strain index	.17[b]	.16[c]
Tax constraint index	-.09	-.12
Adjusted R^2	.57	.41
Number of cases	161	161

[a]Significant at .001 level or less
[b]Significant at .01 level or less
[c]Significant at .05 level or less

of Proposition 13 and a host of other related measures, tax limitation laws on the books apparently had relatively little effect on the traditional revenue policies at which they were aimed.

The results in Table 4 can be compared with those in Table 5, which shows the results of a multiple regression in which the most promising predictors of nonguaranteed debt (from Table 3) are considered conjointly. In contrast to taxation and traditional borrowing, variation in nonguaranteed debt is primarily a function of fiscal strain, not background variables such as total size and functional scope. Both dramatic population change (on either the growth or the loss side) and, to an even greater extent, the fiscal strain index are significant predictors of outstanding nonguaranteed debt, suggesting

Table 5. Explaining Patterns of Per Capita Nonguaranteed Debt, 1977

Predictor Variables	Standardized Regression Coefficients
Squared population change	.19[a]
Fiscal strain index	.45[a]
South	.20[b]
Northeast	-.17[c]
Tax constraint index	.14[c]
Reformism index	-.01
Adjusted R^2	.41
Number of cases	161

[a]Significant at the .001 level or less
[b]Significant at the .01 level or less
[c]Significant at the .05 level or less

that involvement in this newer form of indebtedness is very much driven by shorter-term fiscal pressures and the city's adaptation or failure to adapt.

There is also a contrast to taxation and general obligation borrowing here in that the tax constraint index, while less important than the fiscal strain measures, is a significant predictor of outstanding nonguaranteed debt. Evidently, while the weaker forms of tax constraint that were on the books immediately prior to Proposition 13 had no substantial capacity to lessen per capita tax revenues or outstanding general obligation debt, they were associated with increased city efforts to fund projects through the alternative mechanism of revenue bonds.

The reformism index is unrelated to outstanding nonguaranteed debt once these other variables have been taken into account, while regional effects remain significant. However, region and form of government are confounded. The mean reformism index score (on a scale of 0 to 3) is only 0.8 for northeastern cities, contrasted to 1.3 for midwestern cities and a little over 2 for both southern and western cities. To explore the possibility that region variables may be absorbing the explanatory power that reformism might be expected to have, the regression was rerun without regional dummy variables. Still, the reformism index does not emerge as a significant predictor of nonguaranteed debt outstanding (beta = 0.09; significance = 0.18). In short, there is no evidence to support the contention that business dominance and receptivity to economic development concerns led reformist cities to outstrip less reformed cities at revenue bond usage. Instead, there is evidence that, apart from reformism, revenue bonds were used heavily to finance the 1970s' push for a modernized, economically developed New South; and there is evidence that, for reasons that are unclear, the Northeast lagged behind other regions in use of this fiscal instrument.

A Replication of the Analysis: 1981

Among the more notable findings of this analysis of 1977 data is the relatively poor showing of the political structure variables relative to background variables (in the case of tax revenue and general obligation debt per capita) and fiscal strain (for all three dependent variables, but especially nonguaranteed debt). Neither the debt constraint index nor the reformism index is a relevant predictor of any of these fiscal policies; the property tax constraint index is not a significant predictor of either tax revenue or general obligation debt per capita, although it is of modest importance in accounting for nonguaranteed debt per capita.

It could be argued, however, that, apart from imperfect measurement, the choice of reformism and legal restrictions on public finance condemns the political structures explanation to limited importance. On the one hand, reform versus nonreform distinctions that once may have had considerable meaning have surely faded. It is not only that mixtures of reform and nonreform elements have become more and more common; it is also that a breed of new fiscal populist mayors has emerged from broken-down New Deal coalitions (Clark and Ferguson, 1983), and these officials are as fiscally conservative, business-oriented, and committed to economic development as their counterparts in reformed settings; in fact, they may be virtually impossible to distinguish from the classic city manager functioning in a highly reformed setting.

If the reformism index represents a political structure variable that had worn thin by 1977, perhaps the 1977 tax constraint index represents one whose time had not yet come. Beginning in about 1978, a fiscal limitation movement dramatically changed the situation with respect to state-level constraints on local taxing in many locations. This movement introduced many new vehicles for con-

Table 6. Explaining Patterns of Per Capita Tax Revenue, General Obligation Debt, and Nonguaranteed Debt, 1981

	Standardized Regression Coefficients		
Predictor Variables	Per Capita General Obligation Debt	Per Capita General Obligation Debt	Per Capita Nonguaranteed Debt
1980 population	.36[a]	.21[b]	—
Functional scope index	.44[a]	.17[c]	—
Northeast	.19[b]	—	-.30[a]
West	—	-.29[a]	—
South	—	—	-.06
Squared population change	—	—	-.002
Fiscal strain index	.02	.13	.58[a]
Reformism index	—	—	-.03
Tax constraint index	-.11	-.15	-.006
Adjusted R^2	.53	.27	.35
Number of cases	133	133	133

[a] Significant at .001 level or less
[b] Significant at .01 level or less
[c] Significant at .05 level or less

straint, such that by 1981 the ACIR no longer reported some bits of information that were used in the 1977 property tax constraint index, while newly relevant information began to be reported. This includes information on whether municipalities are subject to limits on assessment increases, a feature that applied in six states by 1982, half of them having adopted this limitation since 1977. Also by 1982, full disclosure requirements applied to municipalities in nine states, five of them having adopted it since 1977.[11]

An updated, 1981 version of the 1977 analysis is called for, then, in part to provide a more timely estimate of the relevance of tax constraint as a political structure. In addition, an updated analysis may help to shed light on the regional differences observed in the 1977 analysis. If the regional variables represent the traditions and accommodations of the city's long-term history, they should presumably endure in the 1981 analysis. Regional effects may, however, represent evidence of diffusion of fiscal innovation, at least with respect to nonguaranteed debt; if so, such regional effects may have died out by 1981, when this innovation had become more widely used.

Table 6 presents the results of regression analyses for the 1981 levels of each of the dependent variables, using correspondingly updated versions of the independent variables introduced previously.[12] For reasons outlined above, a substantially new type of property tax constraint index is required. The index developed for this purpose is an additive one, composed of a count of the number of the following limitations that applied to the municipality in 1981, according to information presented by the ACIR (1983): (1) specific millage rate limits, applying only to narrowly defined services; (2) overall limits on the aggregate local government tax rate; (3) a property tax levy limit; (4) limit on assessment increase; and (5) full disclosure requirement.

Table 6 shows that, with respect to taxation and general obligation debt, regression results for fiscal year 1981 essentially replicate those for 1977. Background variables, such as city size and functional scope, that predominated in accounting for these in 1977 still remain, as do regional effects. The fiscal strain index, of only modest importance in 1977, falls to insignificance in 1981.

With respect to nonguaranteed debt, there appears to have been greater change since 1977. Fiscal strain remains the paramount predictor, but only in Clark and Ferguson's adaptive sense (1983), as reflected in the fiscal strain index; dramatic population change is an insignificant predictor of nonguaranteed indebtedness in 1981. Cities that were subject to the pressures of dramatic population loss or growth in the early part of the 1970s were distinctive in their tendency to turn, in part, to infrastructure investments backed by revenue bonds. But by the latter part of the decade, revenue bond fever had caught on more generally, and cities experiencing dramatic population change were no longer distinctive in their accumulation of nonguaranteed debt.

Likewise, while the Northeast still trails other regions in revenue bond debt, the South no longer stands out as a uniquely heavy accumulator of this form of debt in 1981. The distinctively high levels of revenue bond indebtedness that were obtained in 1977, perhaps reflecting the push to build the infrastructure for an economically advanced New South, are now matched by similarly high levels elsewhere.

The key reason for updating the 1977 analysis, however, is to reconsider the role of the tax constraint measure in light of the new forms of constraint enacted in the wake of the tax revolt. There is some limited evidence of the intended impacts of the new tax constraint. While not achieving the standard .05 level of significance, the negative property tax constraint coefficients in the regression models for taxation and general obligation debt in 1981 are significant at the .089 and .06 levels respectively, giving some continuing life to the tax cutters' hope that the new forms of constraint would hold back the tide of taxes and tax-backed borrowing. However, Table 6 shows that post-1978 tax constraint is a completely insignificant predictor of nonguaranteed debt. It would appear that, contrary to the hypothesis, these constraints have not had the effect of escalating city dependence upon alternative finance instruments such as revenue bonds. At least, cities subject to stricter tax limitation have not been *visibly* accumulating more revenue bond indebtedness. The possibility of an underground level of reliance on revenue bonds is discussed below.

Discussion

The study of urban public finance has been characterized by an emphasis on the expenditure side and, as Peterson notes (1981, pp. 9–10), by fruitless efforts to isolate political structure variables that have substantial explanatory power.

> If one governmental characteristic has no explanatory power, then the analyst need only search for a means of quantifying another . . . [but] a second problem has been the difficulty of incorporating within the bargaining model the numerous instances when "environmental" variables . . . account for much of the variation.

At first blush, the findings here would appear to be yet another affirmation of the importance of environmental variables and the comparative irrelevance of political structures. But let us consider the matter more carefully.

Comparative analysis of nonguaranteed debt as against taxation and general obligation debt reveals important differences, suggesting a two-tiered model of the revenue side of urban finance. The first tier is the realm of the more traditional fiscal instruments—taxes and general obligation borrowing, which are largely influenced by longer-term factors that must be treated as givens, such as total population, functional scope, and regional location. None of the political structure variables introduced has marked explanatory power in this realm. Even when the more elaborate constitutional and statutory constraints of the post-Proposition 13 era are considered, we find only limited evidence of impacts on taxing and traditional borrowing. The first tier of the revenue picture, like a base budget, reflects long-standing accommodations to powerful economic and demographic realities and to the givens of state versus local allocations of functional responsibility.

Revenue bonds are important because they represent an important, even predominant, part of the second tier of the revenue picture—the realm of choices and adaptations above and beyond the baseline picture established by accommodation to longer-term forces. This second tier is a strategic arena, in which city officials maneuver to adapt to immediate fiscal strain. The adaptation need not involve painful choices about expenditure cuts; rather, it can involve the funding of projects through creative financing arrangements such as revenue bonds. An inclination to pursue such alternatives does not appear to be contingent upon the city's level of reformism. Rather than providing evidence for an iron triangle of reform government, business-oriented development interests, and revenue bond usage, the analysis is consistent with Peterson's argument (1981) that development is a unitary interest of cities generally.

At least in the initial pre-1978 period of escalating nonguaranteed debt, an important political structure variable did help to account for variation in this form of indebtedness. Cities that were subject to comparatively more stringent tax limitation measures were at the forefront of the revenue bond revolution. Curiously, the analysis also shows that the more dramatic forms of tax limitation that emerged after 1977 do not appear to have had the same effect. However, an important caveat must be introduced in this regard: it is possible for cities to be heavily involved in revenue bond-financed projects without appearing to be so, simply by creating special purpose, off-budget enterprises (OBEs).

> These entities masquerade under a variety of disguises; they are called districts, boards, authorities, agencies, commissions, corporations, and trusts. . . . Their financial activities do not appear in the budget of the government unit or units that created them. Thus, politicians have been able to make part of the public sector simply disappear by forming separate entities . . . to conduct borrowing and spending activities. (Bennett and DiLorenzo, 1983, p. 34)

Bennett and DiLorenzo (1982) have argued that the proliferation of these OBEs and the revenue bonds that they use has been a direct response to tax and spending limitations. From this perspective, post-1977 fiscal limitation measures have had important impacts, but not necessarily the ones that limitation proponents had in mind: public officials have adjusted to stringent limitation in an even less visible way than in the early 1970s by hiding escalating revenue bond debt in off-budget enterprises.

In principle, examining urban debt practices would require analysis of the nonguaranteed debt of OBEs created by each city, as well as the general obligation and nonguaranteed debt issued in the name of the city itself. Unfortunately, such analysis is currently impossible. States do not monitor or report statistics for OBEs (Bennett and DiLorenzo, 1983, p. 37) and, except for some Bureau of the Census data on a selected set of the largest OBEs, officially reported data on debt levels and information enabling large scale matching of OBEs with the appropriate general-purpose governmental unit is unavailable. If Bennett and DiLorenzo are correct, however, more and more outstanding city debt is accumulating in these OBEs, and, if the census bureau does not provide better data on them, future research on city debt may require original data collection on OBEs to supplement officially reported indebtedness.

In sum, there is some evidence here of the importance of one political structure variable—constitutional or statutory fiscal limitation—and the distinct possibility that this variable would show even more explanatory power were it not for the less visible aspects of recent revenue bond activity. More generally, environmental variables will surely dominate any analysis of traditional urban fiscal instruments, and some political structure variables, like reformism, have apparently been made irrelevant by the changing patterns of city politics. But the importance of other political structure variables is unlikely to be evident if the study of city money is restricted to the more traditional and visible revenue instruments. To the extent that political structures provide constraints or opportunities to which city officials respond, those adaptive responses are likely to involve flexible, less visible, and strategically manipulable fiscal instruments such as revenue bonds.

Notes

1. Clark and Ferguson's *City Money* (1983) is a good example of this point. In this extensive treatment of fiscal strain, taxing, spending, and municipal finance, only a handful of pages deal in any way with city indebtedness.

2. The vast majority of municipal debt (more than 95%) in fiscald 1981–82 is long-term rather than short-term, that is, payable more than one year after the date of issue (Bureau of the Census, 1984). This research focuses on the different forms of long-term debt; short-term debt is excluded from the analysis.

3. A random sample of 250 of the 831 municipalities or townships with a 1970 population of at least 25,000 was drawn. In much of the analysis, the *N* falls considerably below 250 because of substantial missing data, especially with respect to political structure variables such as the composition of the local legislative council. Data on full-faith-and-credit and nonguaranteed debt of cities was obtained from the Bureau of the Census, 1979. Similar data for fiscal 1981 were obtained either from the Bureau of the Census, 1983 or, for smaller communities, from the census bureau's data tape for the 1982 Census of Governments, the latter also reporting fiscal 1981 information.

4. The research focuses on volume outstanding rather than volume issued in a given year because the latter would make the research too dependent upon the vagaries of the year selected for data collection. Particularly since long-term municipal debt frequently involves terms till payoff that are considerably longer than one year (10-year terms are not unusual), volumes of general obligation and nonguaranteed debt outstanding serve as much more useful indicators of the city's cumulative commitments in these spheres of indebtedness.

5. The functional categories used here are similar, but not identical, to those used by Liebert (1976). Liebert, for example, had separate categories for federally aided and general assistance welfare and included a judicial function category. The choice of categories for use in the functional scope index here was dictated by the expenditure categories used in the Census of Governments in both 1977 and 1982. The convention of counting a function only if the amount spent on it exceeds $1 per capita is one suggested by Liebert (1976, p. 19).

6. The regional groupings used by Wolfinger and Field (1966, p. 474) were adopted here, with the exception that the small number of cities in the border state category were collapsed into the South category.

7. To control for intergovernmental transfers that can allow a city to spend more without extracting more from the local community, total general expenditures minus all state and federal intergovernmental revenues was used as the numerator of the fiscal stress index.

8. Estimates of median, owner-occupied housing value and median household income for 1977 were derived from census bureau figures (Bureau of the Census, 1978, 1984) on these variables for 1969 and 1979 (income) and 1970 and 1980 (housing value). In each case, the estimate was derived by adding seven-tenths of the variable's change in value across the decade to the base (value at the beginning of the decade).

9. Cities with missing data on any of the three components of the reformism index had to be excluded from the analysis. Hence, the effective *N* here of 189.

10. Specifically, the property tax constraint index used here is an additive index based upon the following items from the ACIR's (1977) *Significant Features of Fiscal Federalism, 1976–77*: (a) source of constraint (0 = none, 1 = statutory, 2 = constitutional, 3 = both), (b) millage rate limitation (0 = none, 1

= over 10 mills, 2 = 1–10 mills), and (c) ability to exceed rate limits (0 = no constraints, 1 = few constraints, 2 = several constraints, 3 = severe constraints). The borrowing constraint index, on the other hand, is precisely as devised by MacManus (1981) from early 1970s information presented in ACIR (1976); since no updated publication of this sort of information was available, the borrowing constraint index could not be updated. However, it was property taxation rather than borrowing that was the focus of major constitutional and statutory limitation changes in the 1970s.

11. Full disclosure is defined by the ACIR (1984, p. 147) as "a procedure designed to promote public discussion and political accountability requiring local governing bodies to advertise and hold public hearings on proposed tax rate increases."

12. The functional responsibility index was updated using expenditure data for 1981. Percentage population change from 1975 to 1980 (again squared) replaced the 1970 to 1975 change variable, and 1980 total population was used instead of 1975 total population. Finally, the fiscal strain index was updated by using 1981 local expenditures in the numerator; the denominator consists of the summation of two elements: 1980 housing value multiplied by property tax revenue as a proportion of all local revenue and 1979 median household income multiplied by non-property tax local revenues as a proportion of all local revenue. Finally, 1981-to-1982 information to update the reformism index was available for the constituency size per representative component and the form of government component; however, updated information on partisanship was not available, so the cities' status on this component as of 1977 was retained in the updated index. The larger number of missing data cases on this updated reform index, however, accounts for the still further lessening of *N* in the 1981 analysis.

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The following articles, controversy and research note have been tentatively scheduled for publication in the March 1987 issue:

Jonathan Bendor and Dilip Mookherjee. "Institutional Structure and the Logic of Ongoing Collective Action"

Richard L. Hall. "Participation and Purpose in Committee Decision Making"

Seif M. Hussein, Bruce Bueno de Mesquita, and David Lalman. "Modelling War and Peace."

Mark S. Kamlet and David C. Mowery. "Influences on Executive and Congressional Budgetary Priorities, 1955–1981"

Douglas Madsen and Peter G. Snow. "Recruitment Contrasts in a Divided Charismatic Movement"

Clement Henry Moore. "Prisoners' Financial Dilemmas: A Consociational Future for Lebanon?"

Peter Ordeshook and Thomas Schwartz. "Agendas and the Control of Political Outcomes"

Benjamin I. Page, Robert Y. Shapiro, and Glenn R. Dempsey. "What Moves Public Opinion?"

T. Wayne Parent, Calvin C. Jillson, and Ronald E. Weber. "Voting Outcomes in the 1984 Democratic Party Primaries and Caucuses"

Kenneth A. Shepsle and Barry R. Weingast. "The Institutional Foundations of Committee Power"

Peverill Squire, Raymond E. Wolfinger, and David P. Glass. "Residential Mobility and Voter Turnout"

Aaron Wildavsky. "Choosing Preferences by Constructing Institutions: A Cultural Theory of Preference Formation"

CONTROVERSY

DOES HEAVY TURNOUT HELP DEMOCRATS IN PRESIDENTIAL ELECTIONS?

There is conventional political wisdom that high voter turnout in a U.S. presidential election advantages the majority party. Because the Democratic party has been the dominant party in recent decades, this turnout advantage is often believed to accrue to Democratic presidential candidates. In an article in the June 1980 issue of the Review, *James DeNardo challenged this conventional view. Indeed, he claimed that the majority party was likely to suffer with increased turnout when the behavior of core and peripheral voters is taken into account. Harvey J. Tucker and Arnold Vedlitz take issue with DeNardo's reasoning and evidence, and DeNardo embellishes and underscores his original case.*

Political practitioners and social scientists have long embraced the proposition that high voter turnout in partisan elections works to the advantage of majority party candidates. In the last several presidential elections the Democratic party has sought to maximize its chances for success through voter registration drives and turnout campaigns. For many, the association between high turnout and Democratic party electoral success has acquired the status of conventional wisdom in U.S. politics.

DeNardo (1980) has argued that this conventional wisdom is incorrect. Moreover, he has claimed to present theoretical and empirical evidence to show that, in certain electoral districts, high turnout may actually work to the advantage of Republican party candidates. According to DeNardo, the electoral advantage of high turnout is sometimes the opposite of what most believe to be true.

We have reexamined the theoretical and empirical relationship between turnout and partisan advantage in U.S. presidential elections. We argue that DeNardo's empirical test of his model was severely flawed. A more appropriate test finds little support for his model. We also argue that voting models such as DeNardo's are largely irrelevant to the interests of political practitioners. There are a number of important senses in which high turnout works to the advantage of Democratic presidential candidates.

The Conventional and Defection Models

What we call the *conventional model* links high turnout with Democratic party success through the single most important and powerful concept in voting behavior research: socioeconomic status (SES). The tenets of the socioeconomic linkage between turnout and Democratic party success are relatively straightforward:[1]

1. The Democratic party is the majority party in the United States in terms of popular party identification.

2. Partisan support is linked to SES of potential voters. Individuals with higher

SES characteristics are more likely to support the Republican party; those with lower SES characteristics are more likely to support the Democratic party.

3. Turnout is linked to SES of potential voters. Individuals with higher SES characteristics are more likely to vote than are those with lower SES characteristics.

The conventional model recognizes the Democratic party as the majority party of the population of potential voters. However, the sample of those who actually vote in a given election is not expected to be randomly selected from the population. Low turnout is expected to work to the advantage of Republicans because their supporters, while a minority of the population, are more likely to vote. Thus, low turnout elections provide greater potential for Republican majorities. High turnout is expected to provide the conditions necessary for Democrats to translate their majority status in the population of potential voters into a majority of those who vote in a given election.

DeNardo (1980) does not challenge the tenets of the conventional model as incorrect. On the contrary, he accepts them. His fundamental critique of the conventional model is that it is incomplete. His rival model, which we call the *defection model,* adds the concepts of peripheral voters and vote switchers to the equation. DeNardo notes that others have shown that peripheral voters, those who vote only in high-turnout elections, have weaker partisan loyalties and are more likely to defect than are core voters. He argues that the partisan advantage of high turnout will depend upon the division of core and peripheral voters in a district. To the extent that high turnout stimulates peripheral voters to defect, high turnout will work to the advantage of the minority party. He says this is most likely to occur in predominantly Democratic districts.

To test his model, DeNardo first presents a scatterplot of national turnout rates and the Democrats' share of vote in presidential elections from 1932 to 1976. He characterizes these variables as "dismally unrelated" and argues that empirical data refute the predictions of the conventional model with "disconcerting regularity" (1980, p. 406). DeNardo then goes on to present a cross-sectional analysis of congressional elections. He analyzes empirical data from a sample of congressional districts in seven states for six selected elections between 1938 and 1966. He concludes that large turnout generally works to the advantage of the minority party.

DeNardo also finds from his cross-sectional analysis of congressional elections that Democrats benefit more from a big turnout in Republican districts than Republicans benefit in Democratic districts. However, that conclusion is qualified by the observation that the picture has changed over time because the cores have withered in both parties. As a result, he argues, the relationship between turnout and partisan advantage has become weaker in recent elections.

The Conventional and Defection Models Reconsidered

DeNardo finds the conventional model empirically incorrect, but his assessments of both the conventional and defection models are, in our view, very questionable. A scatterplot of the national turnout rate and the Democratic proportion of popular presidential vote is an inappropriate empirical test of the conventional model. Presidential elections are contested on the basis of state, not national, returns. A negative, positive, or neutral relationship between turnout and Democratic proportion of popular vote at the national level can be consistent with a positive relationship between these variables in most states. DeNardo is quite clear that the relevant units of analysis are election districts. However, his purported refutation of the conventional model for presidential elections does not employ election districts.

We believe DeNardo's cross-sectional analysis of congressional elections is also flawed, but for a different reason. A cross-sectional model defines high and low turnout and proportion of the vote *empirically,* in terms of contemporary national averages. Yet, both the conventional model and DeNardo's defection model define these concepts *theoretically,* in terms of their historical values within a given election district. Both models require longitudinal data and analysis.

An appropriate test of the conventional and defection models requires analysis of longitudinal data from election districts. DeNardo's analysis of presidential elections employed longitudinal data but not election districts. His analysis of selected congressional elections employed election districts but not longitudinal data. Thus, neither analysis provides an appropriate empirical test.

It is possible to test the conventional and defection models with data from U.S. presidential elections.[2] For this analysis we present the correlation between level of turnout and vote for the Democratic party for the thirteen presidential elections from 1932 through 1980 (fewer elections for Alaska, Hawaii, and the District of Columbia).[3]

The conventional model predicts positive correlations between turnout and Democratic vote. The defection model predicts that the correlations will be more strongly positive in Republican states and more strongly negative in Democratic states. We distinguish Republican from Democratic states by two measures: (1) the average Democratic proportion of the presidential vote and (2) the proportion of presidential elections carried by Democratic candidates. Table 1 presents orderings of correlations by these two measures of partisan strength.

Table 1 shows a positive correlation between turnout and Democratic proportion of the two-party presidential vote in 29 election districts and a negative correlation in 22 districts. Most of the largest negative correlations are exhibited by the most Democratic states. These findings meet the predictions of the defection model. However, there are numerous examples of strongly Republican states with negative correlations and strongly Democratic states with positive correlations. Furthermore, state partisanship does not seem to account very well for the overall ordering of correlations.

Scatterplots of these data (not shown) clarify the relationship between the correlations and state partisanship. The strongest support for the predictions of the defection model appears where partisanship is defined in terms of the average Democratic vote. However, that support is due primarily to a group of outliers. Seven southern states exhibit extremely large negative correlations and extremely high average Democratic proportion of the vote. If these outliers are omitted, the relationship between partisanship and correlation predicted by the defection model all but disappears. The scatterplot between correlations and partisanship defined in terms of proportion of Democratic victories does not exhibit the negative relationship predicted by the defection model. The two appear to be unrelated. However, this scatterplot does highlight another sense in which most of the southern states are anomalous. The seven southern states that were severe outliers on average Democratic vote are not severe outliers on Democratic proportion of victories. For these southern states partisanship defined in terms of average vote is quite different from partisanship in terms of winning elections.

The southern states are anomalous in yet another way—the elections in which high turnout occurred. For most states,

Table 1. Correlations between Turnout and State Partisan Strength in Presidential Elections, 1932-1980

Ordered by Average Democratic Vote			Ordered by Proportion of Democratic Victories		
State	Democratic Vote	Correlation	State	Democratic Victories	Correlation
NE	.42	.50	VT	.15	-.17
KS	.42	.28	AK	.17	-.15
VT	.43	-.17	IN	.23	.58
ID	.45	.21	KS	.23	.28
AK	.45	-.15	ME	.23	.10
WY	.46	.42	ND	.23	.25
NH	.46	.11	NE	.23	.50
SD	.46	.11	SD	.23	.11
ND	.46	.25	CO	.31	.52
IN	.46	.58	IA	.31	.11
ME	.47	.10	NH	.31	.11
CO	.47	.52	AZ	.38	.29
IA	.48	.11	CA	.38	.44
UT	.48	.26	OH	.38	.05
NJ	.49	.08	OR	.38	-.45
AZ	.49	.29	VA	.38	-.58
OH	.49	.05	WY	.38	.42
IL	.50	.48	CT	.46	.22
CT	.50	.22	DE	.46	.25
PA	.50	.34	ID	.46	.21
OK	.50	-.32	MI	.46	-.50
NY	.50	-.15	MT	.46	.54
VA	.51	-.58	NJ	.46	.08
MI	.51	-.50	NY	.46	-.15
NM	.51	.30	OK	.46	-.32
NV	.51	.47	UT	.46	.26
MT	.51	.54	WI	.46	-.12
OR	.51	-.45	FL	.54	-.70
CA	.51	.44	IL	.54	.48
WI	.52	-.12	NM	.54	.30
KY	.52	.24	NV	.54	.47
MD	.52	.32	PA	.54	.34
DE	.53	.25	TN	.54	-.71
MO	.53	-.01	WA	.54	.10
WA	.53	.10	KY	.62	.24
TN	.54	-.71	LA	.69	-.73
WV	.54	.22	MD	.69	.32
FL	.54	-.70	SC	.69	-.85
HI	.55	.22	TX	.69	-.84
MN	.55	-.51	AR	.77	-.62
MA	.55	-.15	GA	.77	-.81
NC	.57	-.31	MA	.77	-.15
RI	.58	-.34	MN	.77	-.51
LA	.61	-.73	MO	.77	-.01
TX	.61	-.84	MS	.77	-.60
AR	.62	-.62	NC	.77	-.31
AL	.63	-.78	RI	.77	-.34
MS	.64	-.60	HI	.83	.22
SC	.64	-.85	AL	.85	-.78
GA	.67	-.81	WV	.85	.22
DC	.76	.52	DC	1.00	.52

the high turnout elections were those when turnout was high nationally: the elections of 1940 and 1952 to 1968. However, the southern states exhibit a different pattern. For them, turnout increases in each successive election. The unique pattern of increasing turnout in southern states is most probably rooted in changing registration and election practices. At the same time, these southern states are experiencing a shift in relative party strength from the Democratic to the Republican party. The strongest apparent support of the defection model seems more likely a function of coincident change over time than evidence of short-term partisan defection in high-turnout elections.

A more appropriate analysis of the conventional and defection models for the presidential elections of 1932 to 1980 shows that the correlation between turnout and two-party proportion of the vote for Democratic candidates is positive in some states and negative in other states. Clearly, the conventional model does not apply to all states. Some predictions of the defection model seem to be met by a group of southern states. However, closer analysis suggests that these southern states are anomalous in terms of national patterns of behavior. The relationship they exhibit between turnout and partisan advantage does not demonstrate the linkage between turnout and partisan defection predicted by the defection model.

Turnout and Partisan Advantage: A New Approach

DeNardo's and many other scholarly models of electoral advantage define advantage in terms of proportion of two-party vote. The underlying assumption is that advantage is continuous in nature: a higher proportion of the vote is more advantageous than a lower proportion of the vote. Use of correlation coefficients denotes interest in a linear relationship between two continuous variables.

DeNardo identifies political practitioners and journalists as the most visible adherents of the conventional model. Political practitioners are interested in higher vote proportions, to be sure. However, they are primarily interested in winning elections. For them, the most important election outcomes are discrete, not continuous. They define advantage in terms of winning elections.

Increasing vote share over a historical norm may be neither necessary nor sufficient for winning a given election contest. Political practitioners may be interested in models that define electoral advantage as increasing their share of the two-party vote from 30% to 35% or from 70% to 75%. However, they are even more interested in models that define electoral advantage as increasing their share of the vote from 46% to 51%. When political practitioners say that high turnout works to the advantage of the Democratic party, they may well be speaking of advantage as a discrete concept. The findings of correlation analyses are not entirely relevant to a concept of electoral advantage expressed in terms of winning elections.

We undertook an analysis of turnout and partisan electoral advantage defining each concept in discrete, rather than continuous terms. For each state, each election was classified by a fourfold scheme: above-average turnout, Democratic victory; above-average turnout, Republican victory; below-average turnout, Democratic victory; and below-average turnout, Republican victory. Table 2 summarizes five patterns of relationship between discrete measures of turnout and electoral victory.

The 10 states in which Democrats win a majority of above-average turnout elections and Republicans win a majority of below-average turnout elections provide the strongest support of the conventional model. The 6 states in which Republicans

Table 2. Relation Between Turnout and Electoral Victory, 1932–1980

1. Democrats win majority of above-average turnout elections and Republicans win majority of below-average turnout elections: 10 states, 140 electoral votes.

CA
CT
DE
IL
NJ
NM
NV
PA
UT
WY

2. Republicans win majority of above-average turnout elections and Democrats win majority of below-average turnout elections: 6 states.

AL
FL
MS
OK
SC
TN

3. Democrats win majority of above-average turnout elections and Democrats win majority of below-average turnout elections: 13 states.

AR
DC
GA
HI
KY
LA
MA
MD
MN
MO
NC
TX
WV

4. Republicans win majority of above-average turnout elections and Republicans win majority of below-average turnout elections: 16 states.

AK
AZ
CO
IA
IN
KS
ME
ND
NE
NH
OH
OR
RI
SD
VA
VT

5. Each party wins half of both above-average turnout elections and below-average turnout elections: 6 states.

ID
MI
MT
NY
WA
WI

win a majority of above-average elections and Democrats win a majority of below-average elections seem to support the defection model. However, closer inspection of these 6 states shows that they are southern states in which consistently increasing turnout coincides with partisan realignment. These states do not exhibit the short-term defection in high-turnout years predicted by the defection model.

In 13 states, Democrats win a majority of above-average and below-average turnout elections. In 16 states, Republicans win a majority of above-average and below-average turnout elections. Thus, in 29 states the majority party is advantaged by both high- and low-turnout elections. In 6 states, there is no majority party and no partisan advantage associated with above- and below-average turnout.

Analysis of turnout and partisan advantage as discrete concepts demonstrates that there is no relationship in most states. However, that the relationship does exist in a minority of states meets the predictions of the conventional model, not the predictions of the defection model.

Turnout and Partisan Advantage: State vs. National Focus

We have focused on partisan advantage in state presidential elections. When we turn to the question of partisan advantage in national presidential elections, we must recognize that not all state contests are of equal importance. To assess the relationship between state-level turnout and partisan advantage in the national election, state findings must be weighted by electoral college votes.

We have argued that the southern states that provide the greatest apparent support for the defection model have unique longitudinal patterns of turnout and do not exhibit the short-term partisan defection predicted by DeNardo's model. Nevertheless, if one were to take the positive and negative correlations of Table 1 above as evidence of the substantive relationship between turnout and electoral advantage, it is noteworthy that the 29 states with positive correlations control 276 electoral votes. The 22 states with negative correlations control 262 electoral votes. Even when the strongest case is made for the defection model, correlation analysis suggests that high state turnout gives the Democrats an advantage in winning presidential elections.

The same conclusion results from weighting the state findings for a discrete model of turnout and partisan advantage by electoral votes. The 10 states in Table 2 above that meet the predictions of the conventional model control 140 electoral votes. The 6 states that meet some of the predictions of the defection model control 64 electoral votes. The 13 states where Democrats win a majority of above- and below-average turnout elections control 134 electoral votes. The 16 states where Republicans win a majority of above- and below-average turnout elections control 113 electoral votes. The 6 states where there is no relationship between turnout and partisan advantage control 87 electoral votes. The electoral votes of the normally Democratic states and the states the Democrats win when turnout is above average sum to a majority. The normally Republican states, the states the Republicans win when turnout is above average, and the states that have no partisan advantage associated with turnout *together* control a minority of electoral votes. Thus, even when the strongest case is made for the defection model, discrete analysis suggests that high state turnout gives the Democrats an advantage in winning presidential elections.[4]

Turnout and Partisan Advantage in Perspective

Measurement of the empirical relationship between turnout rate and partisan advantage in U.S. presidential elections is surprisingly complex. Longitudinal data from the appropriate election units, states, are required. Turnout, partisan advantage, and the historical partisan allegiance of a state can each be conceived either as a continuous or discrete concept. High national turnout does not always result in high state-level turnout. Partisan advantage in state presidential elections is not necessarily the same as partisan advantage in national contests for the presidency.

Despite the multiplicity of empirical measures, our analyses yield a consistent set of findings. First, for a majority of states neither the predictions of the conventional model nor those of the defection model are met. Second, a larger number of states controlling a larger number of electoral votes meet the predictions of the conventional model than meet the predictions of the defection model. Third, on closer analysis, the states that provide the strongest apparent support for the defection model do not exhibit the pattern of short-term partisan defection predicted by the model.

DeNardo gave his article the subtitle "The Joke's on the Democrats" to highlight his contention that, contrary to conventional wisdom, large turnout works to the disadvantage of Democratic party candidates. More appropriate analyses demonstrate that, although the conventional model is not appropriate to all states, it is not incorrect. For a number of states controlling a large number of electoral votes, high turnout has been associated with Democratic state victories. There are many senses in which high state and national turnout have helped Democratic candidates win the presidency. It is the defection model that is infrequently, if ever, supported.

The conventional model is unacceptable as a comprehensive description of patterns of turnout and partisan advantage in presidential elections. Yet it appears to be a plausible model to guide election strategy for state and national party officials. However one defines turnout and partisan advantage, Democratic presidential candidates have been unquestionably advantaged by high turnout in a number of states typically viewed as keys to Democratic victory. High turnout is strongly related to Democratic success in states such as Illinois, California, Pennsylvania, Connecticut, and New Jersey. In this narrower strategic sense, the conventional model has been consistently valid since 1932. High turnout alone cannot guarantee Democratic victories in state or national election contests. Nevertheless, there are good reasons for the Democratic party to work for high turnout in a minority of targeted states and in the nation as a whole.

The bivariate conventional model linking high turnout to majority party electoral advantage is simplistic and incomplete. There are good reasons to seek more sophisticated and successful models of aggregate electoral behavior. However, those reasons do not include the unequivocal empirical failure of the conventional model. For some important interests and in some important senses, the conventional model and conventional wisdom are correct.

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Tucker and Vedlitz deserve kudos for providing new evidence about the enigmatic connection between turnout and the two-party vote and for raising political and statistical questions that have broad relevance to students of voting. Unfortunately, their treatment of the evidence only obscures what is a beautiful and intriguing electoral relationship. In this note I want to show that their negative conclusions are not really grounded in data at all but follow instead from an analytical strategy that discards information whenever it appears unfriendly to the conventional wisdom.

Why the Relationship between Turnout and the Vote Is So Elusive

The commonplace notion that a big turnout helps the Democrats rests on a belief that the voting electorate becomes more Democratic as it gets bigger. While it is plausible to believe that turnout affects the partisan composition of the electorate, it is a mistake to think that the story ends there. What intuition fails to grasp is that swings in turnout also disturb the rates of defection across party lines, thereby distorting the conversion of partisan strength inside the electorate into the final vote. Once this basic complication is recognized, the next problem is to see that the combined impact of the two mechanisms varies with the partisan coloration of the eligible population. Without careful statistical controls over the factors that condition how the two processes interact, the true relationsip between turnout and the vote disappears in a sea of confounding effects.

The Effect of Turnout on the Composition of the Electorate

First, let's be clear about what the conventional wisdom says. The most common (and naive) version simply assumes that a big turnout helps the majority party. A more sophisticated version considers the quality of both parties' political support. If each party's base in the eligible population consists of a loyal core and a less committed periphery, then intuition tells us that the party with the larger periphery should benefit when turnout goes up. Why? because core partisans turn out consistently while peripheral turnout swings with the circumstances of the political campaign. A rising general rate of turnout therefore signals an influx of peripheral voters into the electorate, adding disproportionately to the ranks of the party with the broader peripheral base. Because the Democrats are the party of the "common man"—drawing more support than do Republicans from politically marginal groups, who are least interested in politics, least educated, and least reliable about voting—surely they ought to benefit from a heavy turnout.

The reader may review my article (DeNardo, 1980) for a formal discussion of this reasoning, but one technical point deserves emphasis here. It turns out that the party with the larger *fraction* of peripheral followers—not the larger absolute number—improves its share of the voting electorate when turnout increases. The point is significant because it means that the benefits of rising turnout do not accrue automatically to whichever party is the majority in a particular state or district. Rather, the Democrats will benefit from a heavy turnout even when they are a minority of the eligible population so long as their base contains a larger fraction of peripherals than does the Republicans' base. The naive version of the conventional wisdom is therefore logically untenable while the sophisticated version makes logical sense.

The Defection Effect

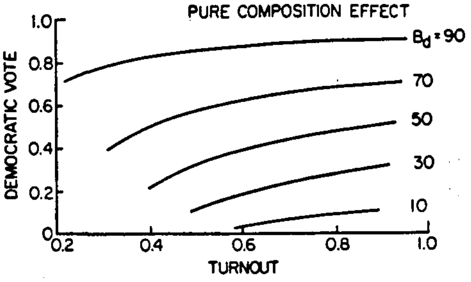
Now if everyone who turned out voted a straight party ticket, the case for the old saw would be logically airtight. But, of course, everyone doesn't vote strictly along party lines. We know that crossover and spit ticket voting happen all the time and that fickle peripheral voters are more easily induced to defect than the loyal core. But if a heavier turnout raises the concentration of peripherals in the electorate, then the normal rates of defection should increase with turnout. The point is crucial because the higher defections will not be politically neutral even when they are visited equally on the two partisan camps. Instead, they will favor the *minority* party (at least on average). Imagine a district in which the Democrats are the overwhelming majority of the voting electorate, say 90%. If defections in each party rose from 0% to 10%, then the Republicans would increase their share of the vote from 10% to 18%. If defections rose to 30%, the two-party vote would become 66% to 34%, and so on.

This reasoning reveals why our intuition is both incomplete and seriously misleading. Higher turnouts may well affect the balance of Democrats and Republicans in the voting electorate as the conventional wisdom assumes. But by bringing more peripheral voters to the polls, they set the stage for rising defections that normally work to the benefit of the minority party.

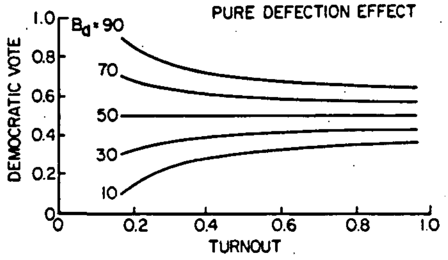
Interactions of the Two Effects

As soon as we accept the idea that peripheral voters defect more readily than do core voters, predictions about the relationship between turnout and the two-party vote must perforce become more complicated than those suggested by the conventional wisdom. While we can predict unambiguously that a big turnout helps the Democrats in Republican strongholds—where both the composition and defection effects work to the Democrats' advantage—the picture is clouded in the Democratic areas where the two

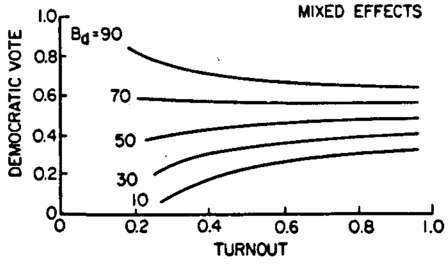
Figure 1. Turnout and the Democratic Share of the Vote



$\theta_c = .85; P_d = .80; P_r = .30; \rho_c = 0; \delta = 0$



$\theta_c = .85; P_d = .80; P_r = .80; \rho_c = 0; \delta = .4$



$\theta_c = .85; P_d = .80; P_r = .65; \rho_c = 0; \delta = .4$

Source: Compiled from Equation 14, DeNardo (1980).

effects pull in opposite directions. There the composition effect favors the Democrats (the party with the larger periphery), while the defection effect favors the Republicans (the minority party).

To understand these complex interactions more fully, consider the graphs in Figure 1, which show how the relationship between turnout and the Democratic share of the vote varies with the Democratic fraction of the underlying population (B_d), the fraction of peripheral followers in each party's base of support (P_d, P_r), and the difference in the baseline rates of defection between core and peripheral voters (δ). The top panel shows the composition effect in pure form (where everyone votes along party lines and $\delta = 0$). Notice how a big turnout helps the Democrats in all constituencies, whether Democrats are a majority or not, but at different rates according to the size of the Democratic base and the level of turnout in the election. The relationship becomes steeper in all circumstances as the fraction of peripheral followers in the Democratic base (P_d) grows relative to the Republican fraction (P_r). The middle panel illustrates a pure defection effect (when $P_d = P_r$) and the dramatically different pattern of relationship it creates. Now the minority party everywhere enjoys the benefits of a larger turnout, but more so when turnout is low and the majority party is extremely large. Finally, the bottom panel gives a typical example of what happens when both effects operate at once. As always, a big turnout helps the Democrats in Republican areas, but the picture is mixed in the Democratic strongholds. When the Democrats are only a modest majority in the district ($.50 < B_d < .70$), the composition effect governs the sign of the relationship and a big turnout continues to help the Democrats. After the transition point around $B_d = .70$, the defection effect dominates and a big turnout helps the Republicans. The value of B_d where the sign of the relationship reverses depends on the relative strength of the two effects. As the discrepancy between the size of the two parties' peripheries grows bigger and the difference between core and peripheral rates of defection gets smaller, the sign of the relationship remains positive in progressively more Democratic areas, and vice versa.

The reasoning and evidence I presented previously (DeNardo, 1980) strongly suggest that both mechanisms influence the relationship between turnout and the vote in U.S. elections such that a heavy turnout normally helps the minority party. Nor should this finding surprise believers in the conventional wisdom. After all, both effects are logical consequences of the same underlying cause—the political fickleness of the parties' peripheries relative to their cores. If you're willing to believe that core loyalists turn out more reliably than peripherals, why not that they vote more reliably for their party after they turn out?

Tucker and Vedlitz raise no objections to the logical or political validity of my formal reasoning. Instead, they claim that properly chosen evidence does not support it. Given a clear understanding of what my model says, we're now in a position to consider how one might test it empirically.

On the Selection of Relevant Evidence

Figure 1 shows that to estimate without bias the impact of turnout on the two-party vote, one must control for several variables that condition the magnitude and the *direction* of relationship. Above all, one must control for the size of each party's base of support (B_d or B_r) since even the sign of the relationship depends crucially upon it. To select cases for study without regard to the underlying strength of the parties would be tantamount to sampling from different curves in Figure 1, making it impossible to draw useful inferences about the true pattern of relationship. Ideally, one would also wish to hold constant the relative size of the two parties' peripheries (P_d, P_r) and the normal difference between core and peripheral rates of defection (δ). The challenge facing anyone trying to devise empirical tests of the theory is that none of this information is easy to obtain in U.S. electoral archives. Indeed, the variables P_d, P_r, and δ are unobservable. The problem of achieving statistical control is therefore a delicate one, demanding both good political judgment and detailed theoretical knowledge. Let me explain how I proceeded and then reply to the dismissive comments of my critics.

The evidence analyzed in my article had been assembled previously from rarely studied publications of the states. This unusual collection of information about congressional districts described not only the rates of turnout among registered voters and the Democratic share of the two-party vote (easy to get) but also the partisan division of registered voters in each district (hard to get). The latter information made it possible to solve three critical problems at once. First, one could construct homogeneous subsamples of districts at six different levels of the crucial conditioning variable B_d. Second, one could measure turnout as a fraction of the population that the variable B_d actually described (here the registered electorate). And finally, there were a sufficient number of cases that each of the subsamples could be drawn from a single election year. In all, I analyzed six separate subsamples in each of six years.

Restricting each of the subsamples to cases from a single year provided valuable indirect control over the remaining conditioning variables in the model. Obviously, no public records tally the relative size of the peripheries in the two parties or the base rates of defection among core and peripheral partisans in congressional elections. Nonetheless, it seems plausible that in a *single year* these quantities should remain relatively stable across congressional districts, particularly among districts that had similar fractions of Republicans and Democrats in the registered electorate. These data provide a reasonable hope that the internal structure of the parties and the behavioral patterns of voters would not fluctuate radically over the districts contained in each subsample. When regression analyses confirmed that the Democrats benefited from rising turnout in all of the Republican subsamples, while in 7 of 10 Democratic samples the Republicans did better (though less so than the Democrats in symmetric circumstances, just as predicted), it seemed justified to conclude on both theoretical and empirical grounds that the conventional wisdom was lacking. A big turnout generally helps the minority party.

None of these considerations seems persuasive to my critics. But they provide no reasons why a longitudinal analysis will provide better statistical control over the conditioning variables in the model. Apparently, Tucker and Vedlitz find it self-evident that to learn the effects of rising turnout in a district, we must observe that particular district over time. Their position is tantamount to denying the utility of all cross-sectional studies, as if to say that one couldn't learn anything from a sample survey about the effect of aging on turnout because no one grows older during the interview. To paraphrase their position: A sample survey model defines old and young in terms of contemporary sample averages. Yet, the conventional model defines these concepts theoretically in terms of their values within a single person. Therefore only a longitudinal study of individuals can be appropriate.

A vast body of statistical theory and experience refutes this position completely. What is required for unbiased inference is statistical control over those parameters in the model that condition the relationship between turnout and the vote. By failing to understand that I have taken every feasible precaution to achieve such control, Tucker and Vedlitz disregard some of the best evidence we have on the problem.[5]

Next consider how Tucker and Vedlitz propose to rectify the "severe flaws" of my approach. Instead of considering statistical cross-sections, they calculate the product-moment correlation between turnout and the presidential vote in each of the states over the period between 1932 and 1980. Then they investigate the relationship between the correlations and two measures of the partisan balance in the states. Before we study their results, it is important to understand why this procedure is suspect. First, their analysis provides only weak control over the strength of the parties in each state. The trouble, of course, is that the summary statistics describe elections spanning a fifty-year period. Are we to assume that the fraction Democratic in each state remains roughly constant over this time? If not—and who wouldn't harbor doubts—then the cases in their subsamples would not be selected from a single curve in Figure 1 corresponding to a fixed level of B_d. On the contrary, they might even be sampled from curves with different signs.

Next, consider the procedure used to measure B_d. Rather than measuring the fraction of registered voters who belong to each party, as I did, my critics calculate B_d as the average share of the vote (and then the proportion of wins) for the Democrats. It is easy to see why both of their measures will give biased estimates of the parties' strength in the eligible (or registered) electorate. As I have shown, the two-party vote reflects not only the underlying partisanship of the state but the distortions introduced by the effects of turnout and defections. Consider the bottom panel in Figure 1 again, and study what the average vote would be in states with various levels of B_d. In most cases, the vote is closer to 50% than is the actual partisan division of eligible voters (mainly because defections push the vote toward 50/50). The proportions of wins will produce a bias of another kind, describing as it does an S-shaped function of B_d. Clearly, when either party gets to be 60% of the electorate or more, it will win almost every election that is held. Thus, neither of my critics' procedures provides an unbiased measure of partisanship in the population for which turnouts are calculated.

Finally, it should be clear that samples spanning 50 years will provide virtually no control over the peripherality of the parties' support or the crucial defection variable δ. During the period under study, the U.S. electorate underwent a severe dealignment, the technology of political campaigning changed drastically, party organizations fell into disrepair, the franchise was extended to 18-year-olds, and the electoral rules evolved in ways that favored split ticket voting. Are we to believe that the critical conditioning variables identified by the model must remain stable over 50 years time just because the cases are drawn from single states?

When we add to these difficulties the decision of my critics to summarize the evidence with linear correlation coefficients—even after I had proven the relationships in question to be nonlinear and even though regression coefficients provide more relevant information—students of elections may well understand why their approach is less responsive to the demands of the problem than is my own. The surprise is that they do obtain interesting results despite the shortcomings of their method.

What the Results Mean

In fact, Tucker and Vedlitz's correlations follow exactly the pattern my model predicts. Consider their Table 1, which shows the correlations as a function of the average vote in each state. Tucker and Vedlitz agree that this summary of the evidence provides some support for my reasoning. "However," they say

> that support is due primarily to a group of outliers. Seven southern states exhibit extremely large negative correlations and extremely high average Democratic proportion of the vote. If these outliers are omitted, the relationship between partisanship and correlation predicted by the defection model all but disappears.

The first thing to notice is how Tucker and Vedlitz discard precisely the states that would be most likely to produce a negative relationship (review the bottom panel of Figure 1). Since theory tells us that highly Democratic states will provide the most striking evidence of the defection effect, if it exists, it is disconcerting to see these cases dismissed so casually—and conveniently—as "outliers." But is it true, after setting the southern states aside, that the relationship predicted by my model "all but disappears"? Are there really "numerous cases of strongly Republican states with negative correlations and strongly Democratic states with positive correlations"? Of the 17 Republican states in the sample, exactly 2 show a negative correlation (−.15 and −.17). And what of the remaining Democratic states? Do they show a consistently positive relationship? On the contrary, if we exclude the only true outlier in the sample (the District of Columbia), then 11 of the 21 remaining Democratic states show a negative relationship (8 of the 11 with negative correlations exceeding .31). Were we to include the southern states, then 18 of 28 Democratic states show a negative relationship. But this is *precisely* the pattern predicted by my model. The bottom panel in Figure 1 shows that a mixture of the composition and defection effects should produce consistently positive relationships in Republican areas, consistently negative relationships in heavily Democratic areas, and mixed results in moderately Democratic areas. Tucker and Vedlitz obtain negative correlations in fully 64% of the Democratic states in their sample. Does this pattern conform to the conventional wisdom? Would anything about the conventional wisdom lead them to anticipate that a big turnout has helped the minority party in 73% of the states?

My critics are not impressed easily. They press their case for the conventional wisdom by throwing more evidence away. First, they discard their percentage of the vote variable in favor of proportion of wins for the Democrats. Now the "seven southern states that were severe outliers on average Democratic vote are not severe outliers on Democratic proportion of victories." Of course not. That's because even states with a mild Democratic majority will produce a high proportion of Democratic victories. In effect, the new measure wastes more information about the underlying division of partisan loyalties in the states by suppressing differences between modestly and heavily skewed electorates.

Finally Tucker and Vedlitz declare that winning is all that matters in electoral politics (notwithstanding much literature to the contrary) and proceed to collapse their continuous variables into dichotomies. By discarding a large fraction of the remaining information in their sample, they succeed in obscuring the last vestiges of the patterns I had predicted. Of course, even if one were strictly interested in winning, it would be a mistake to throw away the continuous information in the sample before estimating the relationship between turnout and the vote.

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Notes

1. See, for example, Campbell and Miller (1957), Campbell, Converse, Miller, and Stokes (1960), DeVries and Tarrance (1972), Key (1966), Milbrath and Goel (1977), and Verba and Nie (1972).

2. We would argue that longitudinal analysis of congressional elections by district is severely complicated by reapportionment and presidential elections. In recent years, congressional districts have been reapportioned every 10 years. The five available elections are an insufficient data base to assess the relationship between turnout and partisan advantage in congressional elections and to control for the impact of presidential elections on congressional election turnout.

3. DeNardo analyzed the elections of 1932 to 1976. Inclusion of the election of 1980 does not affect the findings reported in this paper.

4. We also questioned whether, in those states in which one party won a majority of above- and below-average turnout elections, the minority party won a higher proportion (although less than half) of above-average turnout than below-average turnout elections. Of the 13 "Democratic majority" states, Republicans won a higher minority of above-average turnout elections in 4 states with a total of 57 electoral votes. Of the 16 "Republican majority" states, Democrats won a higher minority of above average elections in 8 states with a total of 55 electoral votes. Thus, the advantage of above-average turnout to the minority party in a minority of elections is balanced between the Republicans and Democrats. Overall, above-average turnout still works to the advantage of the Democrats.

5. Their argument against the presidential data I consider is gratuitous. Since I explain at length why such evidence does *not* constitute an appropriate test of my model (DeNardo, 1980, p. 412), to call my treatment of these data a "test" completely misrepresents what I said. The important point to notice is that my arguments against these longitudinal data apply equally well to the data of Tucker and Vedlitz.

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The Flight into Inwardness: An Exposition and Critique of Herbert Marcuse's Theory of Liberative Aesthetics. By Timothy J. Lukes. (Selingsgrove, PA: Susquehanna University Press, 1985. Pp. 178. $24.00.)

Science and The Revenge of Nature: Marcuse and Habermas. By C. Fred Alford. (Gainesville: University Presses of Florida, 1985. Pp. x + 226. $24.50.)

Critique, Norm and Utopia: A Study of the Foundations of Critical Theory. By Selya Benhabib. (New York: Columbia University Press, 1986. Pp. xiii + 455. $35.00.)

These books focus on how the Frankfurt tradition of critical theory can contribute to the discussion of some intensely contested questions in philosophy and political theory: How does the aesthetic imagination relate to political activity? How deeply is the project of modern science intertwined with the dilemmas of modern society? How can we develop a defensible normative foundation for political thinking.

Timothy Lukes looks at the question of aesthetics and politics in Marcuse's work, in particular the claim in his last book that aesthetic imagination offers a permanent bulwark against "one-dimensional society" and a potential source of a new, radical political orientation. Lukes is sharply critical of this turn to aesthetics, this "flight into inwardness." He argues that recourse to aesthetic imagination is in no sense necessarily linked to the subsequent emergence of radical political action. More likely—and historically, more frequently—it leads to political resignation and impotence.

Given the way Lukes presents Marcuse's position, he does a good job of showing its weakness and revealing the perennial danger of "a premature integration of the political and the aesthetic" (pp. 162–63), but for anyone who takes Marcuse seriously (which is not the same as thinking he is right), this sort of refutation prematurely cuts off speculation about his work and how it might possibly inform current thought and action. In this light, I find it somewhat surprising that Lukes never mentions either the recent work of poststructuralists and feminists or contemporary discussions of new social movements. Many of the same issues Lukes deals with in refuting Marcuse are being extensively debated in such contexts. For example, Lukes traces Marcuse's failures back to his abandonment of various Kantian dualities, such as subject/nature-as-object and reason/sensibility. Many poststructuralists and feminists are relatively close to Marcuse here, and Lukes would have made his adversary tougher and more interesting if he had allowed such thinkers into the discussion as Marcusean advocates. Had they been brought in, Lukes might have explored, in a more provocative way, the implications of Marcuse's emphasis on a new aesthetic sensibility emerging in recent social and political movements, especially among women and radical ecologists.

Fred Alford's book is strong in precisely the way Lukes's is weak. He examines the controversy between Marcuse and Habermas over the nature of science, and asks us to reexamine the stereotypic views of these thinkers in light of the recent ferment in Anglo-American philosophy of science (Kuhn, Feyerabend, Rorty, Hesse, etc.). Although none of these philosophers explicitly endorses Marcuse's idea of a "new science," Alford uses the implications of their work to draw the conclusion that "in some respects Habermas' view of science is less satisfactory than Marcuse's" (p. 2).

Alford's title refers to Horkheimer and Adorno's thesis of the "dialectic of enlightenment," which holds an instrumental (read *scientific*) orientation to the world to be responsible for an attitude of domination whose corrosive effects spread from external nature to internal, human nature, as well as to intersubjective relations. The "revenge of nature" thus refers to external environmental crises as well as social and psychological problems stemming from repressive strategies toward internal nature, in particular those typical of the modern world where the erotic, aesthetic, and playful qualities of human beings are shunted aside by rational and calculative ones.

Both Habermas and Marcuse are deeply influenced by this set of concerns, and Alford examines their philosophical speculations about science from this perspective. His analysis is sophisticated, thoroughly informed, and judicious. He probes with particular care the many changes and ambiguities within the two theorists' work. The richness of this discussion makes it particularly difficult to summarize in a short review.

Marcuse, as is well known, developed the idea of a "new science," which he hoped would ultimately transform our conception of the relation of man to both inner and outer nature. He looked forward to a time in which a more *nonrepressive attitude to internal nature* would allow Eros to become a "builder of culture," and in which our relation to external nature would be characterized by "sensuous perception," not an abstractive, instrumental one (Alford, pp. 39–58). Alford does not in any literal sense defend Marcuse's "new science," but rather asserts simply that he admires its "utter radicalism" and implicit recognition of what the theoretical pluralism of the new philosophy of science tells us: that grand philosophical speculation about science "should stem from free and playful thought *and should be recognized as such*" (pp. 15, 171–73).

Marcuse's speculations about science have typically met with skepticism because they appear to involve a wildly utopian notion of universal reconciliation of man and nature. Habermas's route to solving the problems of the "dialectic of enlightenment" is usually seen as more plausible. Although his precise strategy has evolved, his underlying intention has been to accept, on the one hand, that man's relationship with nature is unavoidably instrumental, but to demonstrate, on the other hand, that this orientation has become unnecessarily imperialistic in modern society, submerging our capacity for normative and aesthetic-expressive rationalization.

What disturbs Alford and other recent critics is that Habermas's conceptual scheme of types of knowledge (theoretical, normative, aesthetic-expressive) is too rigid, in the sense that it arbitrarily sets up limits to the kind of conceptual revision that should result from taking account of "radically new categories of experience" (p. 43). The last and best chapter of the book tries to establish this claim. Alford tries to undermine Habermas's contention that an instrumental attitude to nature is the only one that is "*theoretically fruitful*" (p. 148). He makes use of anthropological examples of "non-instrumental healing (and harming) methods" to establish the point that, at least with regard to internal nature, we can have experience which is not very easily accommodated within Habermas's conceptual scheme (p. 152 ff). Although I think that Habermas could in fact deal with these examples more easily than Alford would allow, the discussion is nevertheless provocative and focused on key questions.

Benhabib's focus is the normative foundation of social theory and political practice. She states in the preface that she had originally intended to examine the tradition of critical theory, beginning with Hegel's critique of Kant, in order to show that Habermas's "communicative ethics" was an untenable foundation. In the course of this project, however, she became convinced that Habermas's main ideas were correct. The key question then became what further insights could be gained from sources such as Hegel for the task of further developing the insights of communicative ethics.

This initial standpoint gives Benhabib's book a somewhat strange quality, for her exhaustive rethinking of the tradition of critical theory leads her to endorse, one after another, each of the major points Habermas has been making over the last couple decades: that the "work model" of action is inadequate; that the notion of autonomy and the kind of politics that emerges from the peculiar standpoint (adopted by Hegel and successors) of the "collective subject" are flawed; and that the approach of Habermas's predecessors in critical theory was becoming less and less likely to have any leavening influence on mainstream social science.

Although Benhabib thus rediscovers much that was already discovered, one should not conclude that this is a book without much new to say. First, it is useful because of its clear and penetrating discussions of the central philosophical tensions and shifts that have emerged from Hegel to Habermas. Where this book really distinguishes itself, however, is in its incisive discussions of the nature of action and of ways to make communicative ethics more defensible. As regards the concept of action, Benhabib, like Habermas, wants to reorient our thinking away from its focus on subjectivity. She argues that action has certain ontological qualities—its linguistic nature and "interpretive indeterminacy"—that can only be adequately grasped by theories emphasizing "radical intersubjectivity and pluralism" (pp. 55, 87ff, 136ff, and ch. 8).

Although Benhabib's insights run generally parallel here to Habermas's, she develops them independently enough that she can employ some of them against him in a critical fashion. For example, she worries that his insights about intersubjectivity and pluralism are threatened at those points in his theory where he employs transcendentalist figures of thought and speaks of a reconstruction of the history of the species' competence for moral thinking. Although I would argue she overplays this threat, Benhabib is clearly correct to be concerned and to draw attention to the proper order of values in communicative ethics. Her insights on these issues are also supplemented by a very good analysis of the problem of moral judgement, and of how communicative ethics must open itself more to what feminists such as Carol Gilligan have been saying about this topic. In sum, Benhabib gives anyone interested in communicative ethics and politics some fresh ideas on how, with Habermas, we can think beyond the philosophy of the subject, without running blindly into the embrace of post-structuralism.

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Constitutional Choices. By Laurence H. Tribe. (Cambridge: Harvard University Press, 1985. Pp. xiv + 458. $29.95.)

God Save This Honorable Court: How the Choice of Supreme Court Justices Shapes Our History. By Laurence H. Tribe. (New York: Random House, Inc., 1985. Pp. xii + 171. $17.95.)

Politics, Democracy, and the Supreme Court: Essays on The Frontier of Constitutional Theory. By Arthur S. Miller. (Westport, CT: Greenwood Press, 1985. Pp. viii + 368. $35.00.)

Tribe and Miller seek a new legal realism, or contextualism, in the making of constitutional choices and reject much of modern constitutional theory and practice for failing to protect rights effectively and to limit the abuse of public and private power. They want the Court to make choices based not on a priori principles that endure, but on 1) political-structure values and rights values in the context of each case and 2) on impressions about the nature of political, economic, and social facts and inequality.

Constitutional Choices consists of 16 superb —at times brilliant—essays, 12 of which have not appeared elsewhere in their present form, that argue against the present trend of grand constitutional theory toward a segmentation into groups of scholars who advocate that the legitimacy of judicial review can only be sustained by staying close to the Framers' intentions (Robert Bork), by proceduralism (reinforcing the structures of representation and stopping racial prejudice, (e.g., John Hart Ely), or by creating rights (Michael Perry).

Tribe describes the themes that cut across the essays: the domination of constitutional theory and practice by "the dangerous allure of proceduralism," "the paralyzing seduction of neutrality," "the morally anesthetizing imagery of the 'natural' in gender, race, and economic inequality relations," "the hidden . . . tilt of various constitutional doctrines towards the perpetuation of unjust hierarchies of race, gender and class," and the potential of various forms of constitutional argument to deflect judicial responsibility from crucial substantive choices onto external circumstances or actors. These essays, which should be considered necessary reading for scholars and students of constitutional law, cover a full range of issues; congressional limitations on Supreme Court jurisdiction, the legislative veto, standing, state sovereignty, interstate bank mergers, freedom of speech, the compensation and contract clauses, affirmative action, gender categories in the law, and state action.

The strength of the book is that it actually shows us one of the foremost constitutional scholars and practitioners at work making constitutional choices—that is, considering the relationship in the Constitution between structural values and rights values in the light of the realities of economic and social power. His analyses result in a most perceptive critique of constitutional theory and the practice of the Burger Court, showing that neither rights values nor representation-reinforcing values alone can inform constitutional choices; nor are they inherently more neutral. He argues that both values must be viewed in tension in all areas of constitutional law. To base one's constitutional choices on only one of these values, or on Bork's interpretivism, creates a priori areas of public law that are by definition either out of bounds or in bounds for the Supreme Court or political institutions. Modern constitutional theory fails to keep alive the problematic nature of judicial and political power.

Tribe would like the Court to make affirmative choices about economic, social, and political realities rather than use what he considers to be falsely neutral categories in the law. If such affirmative choices are not made, then constitutional choices will replicate or increase inequalities through law. In the area of First Amendment jurisprudence, Tribe argues that efforts to secure content-neutral categories; to make distinctions between speech and action; to create different rules for obscenity, commercial and libelous speech, and public and private forums; and to secure equal versus guaranteed access rules—that is, the concern for neutrality over all other values—lead to grossly inadequate protections for the speech of the poor, the politically weak in society, and those with unpopular, less-conventional viewpoints. He wants the court to make substantive choices based on the resources available to individuals, groups, and institutions so it can affirmatively protect all who seek to influence the marketplace of ideas. Also, there has been too much "cost-benefit" balancing of direct effects of speech activities with the diffuse but crucial speech values and with the favoring of allocational efficiency (merely more speech), rather than distributional equity.

Similar arguments are made about equal-protection categories. Tribe argues that because only women can bear children and men now are not required to give a part of their body to sustain the life of a child, the court should affirmatively protect women's right to choose abortion by requiring Medicaid payments for abortions. Rights of privacy, Ely's proceduralism, and Bork's interpretivism do not go far enough in providing equal protection of the law given this physical reality. Also, Tribe opposes society's viewing of the sexual mores of men and women, as groups, as different and "natural" (as in statutory rape cases) because this perpetuates stereotypes about both genders.

In the area of separation of powers, Tribe's contextualism is seen in his rejection of Lochneresque categories, formalisms, and principles used by the Burger court and constitutional scholars because they hide the complexities of institutional power. For example, he rejects the distinctions among judicial, congressional, and presidential power used by the Burger court to outlaw legislative vetoes and to bankrupt the power of the courts. He opposes Supreme Court review of congressional decisions about whether states have retitled a proposed amendment because the amendment process is the way to overturn Court decisions. Tribe also opposes congressional efforts to limit the jurisdiction of the Supreme Court and lower federal courts because such efforts would violate Supreme Court powers under Article III, and substantive rights values in the Bill of Rights.

While Tribe's criticism of present constitutional theory and practice is superb, he has gone overboard in his contextualism. It leads to a rejection of abstract theory itself—to nominalism—which may result in outcomes he opposes: less active discussions about what process and rights values to choose and less consideration of the problematic nature of all uses of power.

The problem in his approach is that there is no unifying theory for deciding such questions as when to grant power to one branch of government over another, when to protect one group or individual instead of another, or when to grant individualized determinations by government. The standards are inductively determined by looking at specific factual situations in light of views concerning (1) political, economic, and social inequalities in our society, (2) general political structure (3) substantive values in the First Amendment; (4) the Equal Protection Clause; (5) and the doctrine of separation of powers that are discovered as cases are decided.

Tribe has made a good case for the lack of neutrality of categories in free speech cases that involve speech conduct, content-neutral mechanisms by government, and the looking at different types of speech with different standards. However, he has not shown that to allow courts and scholars to abandon the search for neutrality, to read basic social and economic facts, and to make ad hoc affirmative rights protection choices is a better way to proceed. The rejection of a priori principled constitutional theory in favor of an inductive, ad hoc process of rights and political structure choices is an open invitation to jurists whose readings of the nature of social and economic equality are different to come up with decisions that may be hurtful to the groups Tribe seeks to protect. Neutral categories (even if not as truly neutral as Tribe would like) may be more protective than a deconstructed contextualism of First Amendment and equal-protection values, for they set limits on the constitutional choices of jurists with more conservative economic and social philosophies.

Tribe's opposition to the use of formalisms in deciding the nature of individual rights or the powers of government undercuts one of constitutional law's most important devices to limit politics and protects rights. The long-term effect of the denial of abstract principles, formality in constitutional law, and the Framers' intentions in favor of institutional innovation, while implicitly supporting the Framers' values such as the need for the independence and accountability of each branch under the doctrines of the separation of powers is to denude constitutional theory of its ability to be prescriptive and educational. It could lead to a non-Euclideanism—a relativism like that which plagued pluralist scholarship of American institutions and constitutional law in the 1950s and 1960s. Relativism then led to a delegitimation of the checking power of constitutional law; to the development of theory without critical principles because its principles came from "what is" and to the failure to create and justify benchmarks for testing whether society met process and rights values in the Constitution.

Ironically, Tribe's argument against modern constitutional theory and in support of doing constitutional law shows the need for more systematic constitutional theory and has within it some of the criteria for such theory. Tribe's important demonstration of the impossibility of separating process and rights values in making constitutional choices could be a central premise of the new theory. It might draw on empirical studies of abuses of power and rights on the national, state, local, family, and individual levels, and of the use of power by the Court versus its use by political institutions. It might be informed by normative theories of rights. It would be a theory with moving categories of process and rights that would suggest how to make tough choices, how the overall scheme of constitutional categories influences the choices made within a specific category, and how the categories resonate with inequalities in social, economic, and political life.

In *God Save This Honorable Court*, Tribe offers well-known anecdotes from history to argue against the conventional wisdom that presidents are surprised about what their Supreme Court nominees do once they get on the Court; that the appointments of specific presidents, even when few in number, have not made a significant difference in the direction of constitutional law; and that the Senate has in the past been spineless in consenting to presidential nominations to the Court.

More interesting are Tribe's arguments for the Senate's taking a more active role in judicial selection. These includes the Senate's greater diversity of viewpoint, compared with that of the president, due to ethnic, class, race, sectional, and party diversity; the greater number of electoral snapshots of public opinion in Congress and the Senate; and the future-looking direction of the Senate and House versus the past-looking views of (lame-duck) presidents.

Tribe urges the Senate to support nominees with complex judicial philosophies and views on both institutional relationships and substantive rights values in the Constitution. They should understand the constitutional questions raised, for example, by the abortion rights case, *Roe v. Wade*, rather than be asked to pass a litmus test of support or opposition to that case. The Senate is asked to make judgments about whether a nominee is within "the outer limits" or boundaries of constitutional or judicial philosophy. For example, the nominee should favor the incorporation doctrine and major cases opposing racial discrimination. Tribe is most concerned about securing ideational diversity on the Supreme Court. He advocates that the Senate should consider the judicial philosophy of a nominee in light of the mix of philosophies already on the Court.

In this book, Tribe's contextualism is evident in his failure to consider theories and systematic empirical studies on legislative behavior. Large representative bodies, like the Senate, with ends-oriented members surrounded by single-issue interest groups, tend to favor compromise. Lowest-common-denominator nominees who are not objectionable to any significant subgroup would be chosen, rather than women or men of strong judicial philosophy with leadership qualities, judicial temperaments, and qualities of mind capable of meeting Tribe's goal of countering the dominant judicial philosophies already present on the Court.

Arthur S. Miller offers 11 essays, only one of which is original to the volume, that are highly critical of modern constitutional theory for its lack of political theory and doctrine, which could make it a strong molder of change in society. Law made by lawyers, rather than by political theorists, allows politics, presidents, and the powerful to dominate society and fails to provide a utopian vision of the future. The potential for change through judicial review is limited because doctrine assumes there is accountability in the political system and concern for human dignity. The problem lies in archaic distinctions such as that between public and private spheres and the separation of powers. Miller's call for new doctrine to curtail presidents' unlimited powers in foreign affairs and war, to create a fundamental right to employment in this age of technological change, to establish democracy in corporations, and to protect the world environment. Although his critique offers a refreshing look at the moderate nature of reform in our pluralist society, Miller fails to provide us with an alternative political or constitutional theory, an internal critique of doctrine (as Tribe does), or a systematic analysis of the law, political institutions, or society that would help us achieve some of his laudable goals.

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POLITICAL THEORY

A Weber-Marx Dialogue. Edited by Robert J. Antonio and Ronald M. Glassman. (Lawrence: University Press of Kansas, 1985. Pp. xxi + 334. $29.95.)

Any book entitled *A Weber-Marx Dialogue* is striking in two respects. First, dialogue is not a word that comes to mind when thinking of Marxist-Weberian relations. Second, Weber's name precedes Marx's in the title. These two things tell us much about the authors of this collection of essays: they are not Marxists. With Marxists, the word *critique* invariably precedes Weber's name, and Marx's name invariably comes first. After all (and this is the conventional wisdom, and not only the Marxist view), Weber was Marx's epigone, a thinker who defined himself over and against his better. Marx was a turning point in intellectual history; Weber an interesting, but in a way inevitable, twist in the road.

The purpose of this collection appears to be, therefore, two-fold: first, to elevate Weber, the thinker, to the level of historical significance occupied by Marx, and second, to end the shouting match by showing that Weber was not merely, or even primarily, Marx's antagonist. Hence, this is not intended as a dialogue in the sense that Marxists are chatting with Weberians, but rather in the sense that non-Marxists are showing the bases on which such a dialogue is possible and desirable. Thus, we have a very complicated maneuver underway here—a prolegomena to any future dialogue, not the dialogue itself.

It is, of course, extremely dangerous to make generalizations about any book containing essays by 13 authors as diverse as Guenther Roth, Wolfgang Mommsen, and Stephen Turner. Each, obviously, has his own agenda, and all have different interests, but all of the essays are bound together by certain recurrent themes. Among the issues addressed are the problem of determination in Marx and Weber; the idealist/materialist controversy; and capitalism, socialism, and their relations to bureaucratization—themes one would expect. What makes these essays interesting is that the authors' responses to these problems are not altogether expected. Steven Turner argues that Weber did not reject Marx's categories or methods altogether, but that "the elements of the Marxian account are retained, as members of the supporting cast in a story Weber regarded as more significant. . ." (p. 185). Working from the other direction, Douglas Kellner argues that Weber had great influence on twentieth-century Marxists—particularly the Frankfurt School—that, far from being acknowledged, was hidden behind a mass of anti-Weberian polemics. Franco Ferrarotti goes so far as to argue that to regard Weber's *Spirit of Capitalism* as an anti-Marxist work distorts it greatly.

We see in these three essays an underlying unity: all argue that the Marx-Weber distinction is not so obvious as one might think, and therefore, that a unified or at least commingled sociology is in fact possible. At the very least, discourse between the two sociologies is not made impossible by any methodological bars. Why, then, has this not happened? Some of the culprits are named in Kellner's essay, and in Johannes Weiss's fascinating survey of Eastern European literature on Weber. Weiss describes the way in which Marxists, with the interesting exception of the Poles, have either ignored Weber or savaged him, accusing him of neo-Kantianism, subjectivism, and moral agnosticism. Thus, the argument emerges that it has been the Marxists that have made dialogue impossible—in the West because of the Frankfurt School's covert and willfully unacknowledged borrowings, and in the East, because of the doctrinaire rigidity of Leninism.

The theme of possible unity is continued in an interesting way in Ira J. Cohen's essay, which criticizes both Marx and Weber for the same failing. Neither, according to Cohen, appreciated the vigor and validity of democracy, but both, for different reasons, tended toward anti-democratic sentiments. Thus, the failure of modern social science to develop a coherent sociology of modern democratic society is laid equally at the doors of Marxism and Weberianism; this is the unity of failure.

Stephen L. Esquith, in the book's final essay, tries to put all this into perspective by arguing that there was a powerful general unity between the works of Marx and Weber, but that, in spite of some general agreements, it would be a mistake "to conclude that . . . all their political differences have been exaggerated." (p. 314) It is not surprising that there were profound methodological similarities in Marx and Weber. After all, both were brilliant pupils of Hegel, working within his developmental paradigm (a point made by Scaff and Arnold). Yet, whatever their methodological affinities, it is too easy to forget the enormous political difference between the two.

This should give us pause about any Weber-Marx dialogue. Whatever Weber's politics were, they were not the politics of a revolutionary socialist. Whatever methodological similarities Weber and Marx might have had, we cannot but believe that their political choices were more important than their methodological choices. We social scientists, in our self-absorption, frequently forget the relative importance of method and politics. Previous literature on this question focused obsessively on methodological questions, and ignored the political. To a great extent, this collection of essays does not suffer from this defect, and for that it is to be praised. While trying to show us a methodological convergence, the authors do not use this insight to lay claim to a new theory of political convergence.

GEORGE FRIEDMAN

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Making Sense of Marx. By Jon Elster. (Cambridge: Cambridge University Press, 1985. Pp. xv + 556. $49.50, cloth; $15.95, paper.)

What of Marx remains valid today? This separation of the chaff from the wheat is the goal that Elster has set for himself in this new book. The task is not an easy one: it requires, besides mastery of Marx's voluminous production, knowledge of the most up-to-date theories concerning all the phenomena that Marx examined, which range from philosophy to sociology, through economics, political science, and history. Elster's achievement on each one of these fronts is remarkable.

Making Sense of Marx comprises a methodological introduction, two substantive parts, and a conclusion in which Elster provides a critical evaluation of Marx's work. The first part deals with issues relevant to philosophy and economics. Marx's views on human nature, social relations, alienation, the philosophy of history, exploitation, freedom, coercion, and justice are presented and critically discussed, together with his theories about technical change, the labor theory of value, the falling rate of profit, and capitalist crises. The most valid of Marx's views today, Elster argues, are those concerning technical change and exploitation. I will not summarize Elster's points on technical change (he has written a whole book on the subject). With respect to exploitation, he argues that the concept not only provides the grounds for normative criticism of capitalism, but also constitutes an explanation for social action. On the other hand, the labor theory of value has a congenital and fatal flaw: it assumes homogeneous labour.

The second part focuses on Marx's theory of economic history and the political issues of class struggle, the state, and ideologies. In the chapter concerning the Marxian theory of economic history, Elster examines the relationship beteen forces and relations of production and Cohen's argument about the centrality of functional explanation in Marx.

In the next chapter he examines the other candidate for the motive force of history: class struggle. Elster examines Marxian texts very closely in order to reconstruct a definition of class, then departs from them in order to examine what he considers the most important problem in the social sciences: why people cooperate. (p. 366) The question is raised from the concept of *class consciousness*, which Elster defines as the possibility of overcoming the free-rider problem. In the last section of the chapter, Elster focuses mainly on the political writings of Marx and examines the questions of class struggle and class coalitions, which he considers important Marxian contributions.

The last two chapters of this part of the book examine questions concerning two important aspects of class societies: the state and ideologies. What is the relationship of the state and ideologies to the dominant class? Is the state the instrument of the dominant class, or is it autonomous? Is the dominant ideology the ideology of the dominant class? The last question provides an additional opportunity for further development of issues relevant to the nature of explanation in the social sciences and of endogenous belief formation.

So far I have summarized the book's systematic presentation and criticism of Marx's thoughts. It would be wrong to conclude from such a summary that the book is simply one more approach to Marx's contributions. If Elster can be criticized, it is not because he has been too servile to Marx, but because he holds strongly his own beliefs and tries to see Marx through them.

In fact, the book is original in several ways. It is concerned not with textual exegesis only (although it does not hesitate to expand in this direction when it is required), but with the internal coherence of Marx's theories, and with the comparison between those theories and reality. It does not deal with the ideas of other writers belonging to the Marxist tradition, Engels, Lenin, Trotsky, Luxemburg, and Gramsci are absent from the book. Instead, references to Cohen and Roemer (the other two editors of the Studies in Marxism and Social Theory series are very frequent. Another original aspect is the author's attempt to strike a delicate balance between defending and criticizing Marx's arguments. No doubt the mix will not please everybody. Questions like, "Then what remains out of Marx?" or "Then why refer to Marx at all?" will be heard from one or the other end of the ideological spectrum. Finally, the book attempts a very interesting synthesis between Marx's thought and rational actor approaches in the social sciences. For all these reasons, the reader has a feeling of participation in the first steps of the creation of a new subfield: the intersection between Marxism and rational choice.

Let us analyze this point. Elster is a methodological individualist (a principle rejected by many Marxists). He believes that social phenomena are in principle explicable in terms of the properties, goals, and beliefs of individuals. Where explanations based on microfoundations are not possible (as in the problem of collective action) a black-box explanation may be temporarily necessary, but "methodological collectivism can never be a desideratum" (p. 6).

How do these methodological lenses apply to the work of Marx? Elster rejects Marxian functional explanations because to be valid, a feedback causal loop from the consequences to the causes has to be specified. In the absence of such a loop, explanations are either incomplete or false. This kind of "teleological" reasoning is very characteristic of Marx's philosophy of history, the theory of development of productive forces, and the theory of the political and ideological superstructures.

Based on this reasoning, Elster criticizes large parts of Marx's work as being either uninteresting or wrong. What remains from such a radical criticism? Elster argues that Marx has offered a fertile framework for analysis of social phenomena. It is a three-tiered scheme: first, there is a causal explanation of desires and beliefs of the actors; second, there is an intentional explanation of actions in terms of these desires and beliefs; third, there is a causal explanation of the aggregate outcome of these actions (note that the outcome might not have been intended by the actors). This third part of the explanation was pioneered by Marx, and it has become so standard in the social sciences that "few would think of referring to it as 'the Marxist method'" (p. 3).

According to Elster, the second part of the explanation—the intentional part—was not very well developed by Marx, whose analyses either do not include strategic interactions between actors, or are pre-game theory in nature. The reason is that Marx was relying very much on functional explanations. However, Elster argues, there are analyses of specific situations (in "The Eighteenth Brumaire of Louis Nardeon") or whole books (*The German Ideology*) written on methodological, individualistic premises.

Finally, the first part of causal explanations, which Elster calls "sub-intentional," was also developed by Marx. Again, the Marxist theory of ideologies lacks microfoundations, but in Marxian economic analyses, different agents get false beliefs because they generalize from locally valid views that hold only with a *ceteris paribus* clause. For Elster, this fallacy of composition "is perhaps the most powerful part of the Marxist methodology" (p. 19). An example is the falling tendency of the rate of profit in capitalism. Each capitalist can improve his situation by introducing labor-saving innovations, but if all capitalists introduce labor-saving innovations, the rate of profit falls. It seems to me that the situation can be accurately described as a Prisoners' Dilemma game between capitalists. Elster, however, develops the argument of endogenous belief formation, and shows instances of this phenomenon throughout the book.

In conclusion, *Making Sense of Marx* deals with Marxism and rational choice. As surprising as it may seem with respect to the intellectual history of the two research programs, it seems reasonable after second thought. After all, Marx himself was a rationalist and an English political economist, as well as a Hegelian and a functionalist sociologist. Elster's book makes the point that there is no intrinsic reason for the two traditions to be so far away from each other. On the contrary, they have much to gain from a mutual osmosis of their research agendas. *Making Sense of Marx* addresses itself not just to the intersection of people interested in Marx and in rational choice, but to their union, and to people who like to read books full of interesting ideas.

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Representation and Responsibility: Exploring Legislative Ethics. Edited by Bruce Jennings and Daniel Callahan. (New York: Plenum Press, 1985. Pp. xvi + 332. $39.50.)

Empirical political scientists who study legislatures have been hesitant to address questions of legislative ethics and morality head-on. Journalists, reformers, and an occasional political theorist have provided most explorations of the topic. Ethics has been avoided, both because political scientists have had bad experiences when they have ventured into advocating reform and because treatments of ethics have frequently concentrated on very narrow questions. *Representation and Responsibility* is an important volume that attempts to move the analysis of ethics to a level broader than a focus on criminality, and it does so in a way that self-consciously attempts to wed the empirical and the normative.

Representation and Responsibility is a collection of essays developed for a series of meetings at the Hastings Center in Ethics in 1982 and 1983. These papers set out

> to explore the broad range of ethical responsibilities that arise in legislative life and in the practice of political representation and to place those responsibilities in the context of recent changes in the electoral system and the legislative environment on both the federal and the state level. (p. xiii)

As with most collections, the usefulness of specific contributions varies considerably. On the whole, the collection is at its best when individual essays link questions of ethics to larger issues of representation. Not all of the essays make the link successfully, however. It is especially troublesome that the authors of the empirical and practical chapters talk past the authors of the theoretical essays on representation and ethics.

The most helpful theoretical essay is "The Theory of Legislative Ethics," by Amy Gutmann and Dennis Thompson. They develop an analytical framework for discussing ethics by starting where they should: with representation. Theories of ethics, as with theories of representation, are "essentially contestable." Gutmann and Thompson aid our understanding by describing an analytical framework that stands up regardless of one's representational standard:

> Legislative ethics cannot tell the representative whether to act as a trustee or delegate. . . . It follows that legislative ethics will grant considerable discretion to legislators in their choices of role and their decisions on policy. . . . The primary focus of legislative ethics thus becomes the legislative process itself, and the chief ethical duties of legislators concern making legislative discretion conform to political democracy. (pp. 173–74)

They argue that ethical behavior can be discussed in terms of autonomy, accountability, and responsibility. That is, legislative activity is said to be ethical to the extent that improper influences are minimized and information about legislative behavior is maximized, and that corporate responsibility for the state of the legislature is also maximized. One can argue over what a "proper" legislative influence is, for instance, but Gutmann and Thompson at least define the salient characteristics of ethical systems and provide a starting point and common standard for contention.

Few of the empirical essays on actual legislative behavior rely on a framework as fine as the one Gutmann and Thompson provide. This probably betrays the limited scope of the legislative research of the last four decades, during which normative questions have been eschewed. Roger Davidson, in his enlightening essay, "Socialization and Ethics in Congress," notes that norms, the most normative concept of legislative research, "did not . . . address larger questions of ethical behavior for elected officials" (p. 117). Whether such ethical norms were simply lacking, were assumed, or were overlooked by political scientists remains unclear. Yet in this volume, the absence of information on ethical norms hinders the empirical and practical writers from engaging fully in the theoretical debate.

Gary Jacobson's essay, "Political Action Committees, Electoral Politics, and Congressional Ethics," demonstrates the keenest coupling of a larger theoretical understanding of ethics with the actual behavior of American legislators. Jacobson's comments also prove to be the most controversial of the volume. Analytically, Jacobson complements Gutmann and Thompson by recognizing that "the most important ethical problems [political action committees] raise are systemic" (p. 42). With Jacobson, as with Gutmann and Thompson, the question of campaign giving turns not on whether money creates an influence, but on the type of influence it creates. If the present system is unethical, it is because narrow particular interests have inproper influence. As Jacobson points out, certain proposed campaign finance reforms would only shift the locus of improper influence (e.g., from special interest groups to incumbent politicians). This leads him to reject most common proposals for campaign finance reform and to note that greater responsibility would only come by strengthening parties—a meager hope.

Representation and Responsibility strikes out in some fruitful directions toward the systematic study of legislative ethics. Those hoping for a unified look at the subject, however, will need to look elsewhere. Those hoping to take up the research agenda on legislative ethics will find this a good place to start.

CHARLES STEWART III

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The Flight From Ambiguity: Essays in Social and Cultural Theory. By Donald N. Levine. (Chicago: University of Chicago Press, 1985. Pp. x + 248. $25.00.)

Professor Levine feels strongly that, for the sake of progress in the social sciences, not to mention the health of humanity, a balance need be struck between precision and ambiguity. He is concerned that our modern existence, having fallen under the spell of Hobbes and Condorcet, lends ever-diminishing value to ambiguous thought and language. His book is an attempt to trace the pitfalls of this evolution, and to stimulate a new tolerance for ambiguity. In his own discipline, sociology, his hero is Georg Simmel, while his antagonist is Emile Durkheim.

Levine became disillusioned with the unambiguous commitment to straightforwardness early in his career. In the 1950s, his lengthy study of the Amhara people of Ethiopia engendered an appreciation of the beauty, complexity, and multivocality of their mode of communication, especially when it came in the form of the enigmatic poetry called "wax and gold." In one of his opening chapters, Levine discusses the multidimensional character of these poems, and argues effectively that the sophisticated, subjective interpretations to which they are open foster a personal autonomy missing in our technological environment. For the sake of "clarity and precision," modernity imposes simple, conformist, tyrannical forms of communication on its constituents, and thus erodes the personal universe.

Levine admits that ambiguity can be an excuse for muddled thinking, or an obfuscation of exploitative motivations, but he also argues that wax and gold has its place in the social sciences, despite the efforts of those like Durkheim to eliminate it. Levine argues that ambiguity must be maintained in the scientific study of society because concepts need to be appreciated for their multiple meanings in order to be understood properly. Here is where Levine discusses theories on the relationship of rationality and freedom in modern society. He highlights Simmel and Toennies as proponents of the argument that increased rationality leads to increased freedom, but also sees them as pioneers in multiplying the meanings of rationality and freedom. He is especially taken by their distinction between objective and subjective forms of rationality.

Levine's discussion turns to Weber, who more than any other commentator, appreciated and refined the multiple meanings of rationality (if not freedom). Not everyone has understood Weber's complexity, however, because many are attracted to his more blatant statements about the diametrical opposition of rationality and freedom. According to Levine, it is not rationality per se that Weber sees as erosive of freedom, but only some manifestations of rationality—particularly the "objective" rationalities in the spheres of economics and politics. Levine, claiming to understand the more complicated (ambiguous) position, argues that Weber recognizes certain kinds of rationality that lead to certain kinds of freedom. For instance, when freedom is divided into "situational" freedom, which involves "external constraints on the movements of actors," and "autonomy," "where actors choose their own ends of action" (p. 165), it can be seen that modern capitalist economies increase situational freedom by using "an open labor market where workers are formally free to sell or withhold their services" (p. 166). Likewise, when rationality is divided into that which is concerned with instrumentalities and that which considers "ends," one sees that a good deal of freedom exists in choosing one of "a variety of technically suitable courses of action, even when a particular end is specified" (p. 168). The obvious problem here, which is not adequately addressed by Levine, is that situational freedom and autonomy are not equally valuable or basic; the same can be said for instrumental rationality and the choosing of ends. Having the "freedom" to choose among an ever-growing number of options while being constrained by a predetermined and non-negotiable end seems to be the same kind of freedom allowed Japanese auto workers when they decide whether to put bumpers or headlamps on new Toyotas. In the end, Weber's concepts of freedom and rationality may not be as ambiguous as Levine would like them to be.

In any case, if we accept that Weber has been simplified and misunderstood regarding the equation of rationality with the curtailing of freedom, it is not all the fault of Weber scholars. Weber himself seems to have resisted the ambiguities Levine discovers in him. There were "idiosyncrasies of his own temperament" (p. 170) that injected undue pessimism into what might have been a more neutral analysis. In a partly biographical chapter on Weber and Freud, Levine argues that "protracted emotional crises" (p. 186) drove the two contemporaries "to exaggerate the extent of repressiveness in modern culture" (p. 197), limiting their ability to articulate and explore their ideas fully. Levine also discusses the intellectual parochialism under which Weber and Freud (and Durkheim) suffered, contracted during their efforts to demarcate independent scholarly discipline. Simmel, on the other hand, who had no interest in acolytes, or academic entrepreneurship, was not so shackled.

Levine assembles his advantages for a concluding synthesis. He has the example of Simmel to strengthen his tolerance for ambiguity. He represents a discipline now firmly entrenched in academia and not in need of intellectually limiting defenses. He does not seem to be undergoing a debilitating emotional crisis, and he has, since Simmel, a multitude of ever-more complicated and specialized treatments of rationality to go on (especially that of Weber). Mustering these forces, Levine "disambiguates" the concept of rationality by articulating and relating its various meanings. He does not claim to have solved the problem of understanding rationality; he only claims that his appreciation of the concept's ambiguity has allowed him to explain the concept better, and prepare it for yet more ambiguous manifestations.

This is not a good book. In very facile ways it links personal "crises" to scholarly positions (concerning Freud, Weber, and Durkheim especially), and thus degrades the independent value of the analyses. It is an eclectic book, bordering on dilettantism. Kant and Hegel are portrayed very simply, jammed between poorly connected discussions of Ethiopia and anomie. The book lauds ambiguity, yet is packed with charts and classification systems, and it criticizes the univocality of modern science without appreciating modern treatments that recognize the scientific engagement as very complex and multivocal. Furthermore, Levine can hide behind his own confusing and disjointed argument by claiming it too represents the scarce commodity: "Surely it is appropriate for a book on ambiguity to harbor a multiplicity of meanings" (p. x).

Yet this is a very good book. It isolates the pitfalls of intellectual specialization and stimulates cross-disciplinary considerations; indeed, the book itself is refreshing in its employment of a variety of disciplinary perspectives. It is a balanced book in that, despite its appreciation of ambiguity, there is an attempt to "disambiguate" concepts—to synthesize ambiguous representations and make sense out of them. The book chastises those who would read and understand the classic literature of sociology simply, and it effectively shows the often forgotten elegance of writers like Georg Simmel.

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Aristotle on Equality and Justice: His Political Argument. By W. von Leyden. (New York: St. Martin's Press, 1985. Pp. ix + 145. $25.00.)

In his preface von Leyden suggests that this, his most recent, book is intended

> to contribute to the discussion of the Aristotelian as well as modern questions of (a) how to render the principle of equality, no less than that of inequality, compatible with the idea of fairness, and (b) how to combine the facts of individual as well as social diversity in civil life with the demands for political justice and cohesion. (p. vii)

In an effort to accomplish these goals, the author uses the first half of his book to discuss Aristotle's understanding of equality as the concept is developed in his political, physical, and metaphysical treatises. The second half of the book contains a general report on Aristotle's analysis of revolution and law, and concludes with the author's summary and evaluation.

As readers of Aristotle know, the question of equality is at times raised in the context of a disagreement between the democratic and oligarchic conceptions of distributive justice. According to the democratic argument, one's equality in respect to his or her free birth should be understood to imply an equivalent equality in respect to all things whatsoever, whereas the oligarchs insist that their superiority in one respect—be it wealth or noble birth—necessarily required a recognition of their superiority in general. Aristotle's "correction" of these two different but equally partial interpretations of justice is his principle of proportionate equality. This principle justified a fair and reasonable inequality of treatment, to the extent that only equals were to be treated equally and unequals unequally. Although von Leyden prefers an Aristotelian theory of proportionate equality, which distinguishes among the worth of various human capacities and achievements, to such an undifferentiated egalitarianism as found in Bentham or Justinian, he nonetheless acknowledges the difficulty of establishing precisely in what respect equals are to be considered equal and unequals unequal. Aristotle argued that the appropriate criterion for differentiating among various functions, and thereby justifying the unequal treatment of citizens in certain cases, is to be found in reference to one's contribution to the common good of society. As some contribute more to the morally good life, so too should they receive proportionately more in return. While accepting this criterion, von Leyden is aware of its difficulties. Although it may not be too difficult to distinguish the level of contribution within a single profession (e.g., to recognize the difference between a good and a poor soldier), it is much more problematic to compare various social vocations that are different in kind (e.g., the contribution of a good farmer as compared to that of a good artisan).

Given the essentially contestable nature of all claims for either equal (democratic) or preferential (oligarchic) treatment, von Leyden argues that there are ultimately only two ways in which such claims finally can be settled: either the resort to revolutionary action or an appeal to law. Accordingly, the final chapter of his book examines Aristotle's treatment of these themes. Von Leyden's discussion of revolution repeats rather faithfully Aristotle's own analysis. The primary task is conceived as an attempt to achieve a proper balance between the interests of the rich and those of the poor, and the solution is presented in terms of establishing the influence of an essentially moderate middle class. Perhaps the most interesting section of this book is found in von Leyden's discussion of Aristotle's theory of law in general and his arguments concerning the natural law in particular. This discussion gathers together several disparate sources and attempts to present a rather full treatment. It is surprising, however, that the author did not provide a more careful analysis of prudence (*phronesis*) or a more detailed discussion of the nature of the morally mature man (*Spoudaios*). Both concepts are absolutely central to any consideration of Aristotle's understanding of the moral order. In general, von Leyden's book repeats the standard analysis of Aristotle's understanding of equality, revolution, and law. Unfortunately, however, it does not attempt much more than this. Indeed, it is not clear to this reviewer what the purpose of the book actually is. As noted, von Leyden indicates certain intentions in his preface, yet these intentions appear to remain somewhat external to the actual analysis within the text, and fail thereby to unify or structure the argument being made. As a discussion of Aristotle's political theory, von Leyden's book lacks the detail and care found in such commentators as R. G. Mulgan, Ernest Barker, or Günther Bien. As a contribution to the contemporary discussion of equality, law, and revolution, its arguments are underdeveloped and indirect at best.

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The Intellectual Development of Karl Mannheim. By Dolin Loader. (Cambridge: Cambridge University Press, 1985. Pp. ix + 261. $34.50.)

Karl Mannheim. By David Kettler, Volker Meja, and Nico Stehr. (New York: Ellis Horwood Limited and Tavistock Publications Limited, 1984. Pp. 169. $11.95, cloth; $4.95, paper.)

As the authors of *Karl Mannheim* observe, Mannheim never gained a place among the major thinkers within the social sciences. He was an early casualty in the war between positivist and antipositivist social scientists, and "it is only as these simplistic oppositions lose plausibility that the undeniable presence of Mannheim at the starting points for the most compelling questions [of social thought] can be acknowledged" (Kettler, Meja, and Stehr, p. 12). If, as it appears, those battle lines have receded and social science has entered into a period of critical self-assessment, then perhaps Mannheim's time of recognition has come. For unlike Aristotle, Locke, or Marx, whose answers must be reckoned with even by those who believe these individuals asked the wrong questions, Mannheim's significance as a thinker depends upon a general willingness to accept the importance of asking oneself probing and troublesome questions in a situation where reassuring answers are not forthcoming. It is the "sensibility and a distinctive openness" (Kettler et al., p. 13) in Mannheim's approach to social problems that makes him such an attractive thinker.

Mannheim's style of writing is itself a reflection of this approach. He did not write books with an Aristotelian beginning, middle, and end, leading to an artificial closure of the argument. Rather, all of his writings—including *Ideology and Utopia* and *Man and Society in an Age of Reconstruction*—are essays or collections of essays. This "essayistic-experimental attitude" (Loader, p. 2) allowed Mannheim to explore a particular problem in depth without worrying about how his arguments could be forced into a linear or progressive alignment. Thus, "to trace his thinking requires some patience and a certain tolerance for essayistic experimentation" (Kettler et al., p. 13). Moreover, Mannheim's belief that intellectuals have an obligation to "think through the problems of social life in terms of the tensions between social strata," as he put it in *Ideology and Utopia: An Introduction to the Sociology of Knowledge* (New York: Harcourt, Brace & Co., 1968, pp. 45-6) virtually guaranteed that he would draw fire from partisans on all sides whenever an exploration of these problems led him—as they invariably did—into the thicket of political controversy. This preoccupation with finding some means for overcoming social conflict by thinking it through to its roots necessarily gives a "political cast to [Mannheim's] thought" (Kettler et al., p. 15). Loader advances a somewhat different view of Mannheim, and I will return to this point later.

Mannheim's earliest writings were focused upon the cultural crisis characteristic of modern industrial society. The organic unity of Western culture had dissolved into atomistic fragmentation and, Mannheim believed, the role of the intellectual was to discover the causes of this crisis and to work toward a restoration of cultural unity (Loader, pp.

10–30). This endeavor might lead one toward a consideration of the rise of science or democracy, or of the socioeconomic causes of conflict, or any of the other issues that comprise the problems that still constitute contemporary social science. Mannheim entered this complex of problems through the door of neo-Kantian philosophy as a critic of the application of positivist methodology to cultural phenomena (Kettler et al., p. 17). Indeed, his discussion of the methodological and epistemological presuppositions underlying an interpretive approach to culture in his essay "On the Interpretation of *Waltanschauung*" (1921) has lost none of its thought-provoking brilliance. Mannheim believed a philosophy of history could supply the values necessary to sustain both this methodology and a cultural synthesis until a deeper examination of the historical and sociological dimensions of culture in the mid-1920s convinced him that the aperture of philosophy provided too narrow a perspective for understanding—let alone resolving—the crisis.

Responding to the dissonant influences of Max Weber, Lukacs, Troeltsch, and Alfred Weber, Mannheim developed a sociology of knowledge approach stressing the role played by social groups in the formation of a particular culture (Kettler et al., pp. 38, 55; Loader, pp. 48ff.). Naturally, this formulation led him toward a consideration of the problem of ideology, which, he believed, lay at "the heart of the problem of political thought in general" (*Ideology and Utopia*, p. 124). Yet, it is precisely in Mannheim's most famous work that the tensions within his thought are most exposed. Not surprisingly, the two books under review differ most sharply in their respective interpretations of *Idology and Utopia*. For Loader, Mannheim is following the model of Max Weber in his attempt to develop a scientific approach to political conflict (pp. 94, 96, 187, 189). As Weber had maintained, the social scientist—including Mannheim—only analyzes social problems; politicians decide what to do about them (Loader, p. 98). *Ideology and Utopia*, Loader argues, is Mannheim's attempt to come to terms not with Lukacs or Marxism, but with Weberian sociology (p. 96). Hence, the discussion of the social role of the intelligentsia in that work ought to be read in the context of Mannheim's application to that group of the scientific ethos of social science as stated by Weber (pp. 119 ff.).

The authors of *Karl Mannheim*, however, give greater weight than Loader does to the fact that Mannheim employs Marx's terminology and assigns a considerable importance to class conflict as a determinant of value positions in *Ideology and Utopia* (Kettler et al., pp. 58, 92, 101). Moreover, Mannheim's critique of the limitations of bourgeois thought and the role he assigns to party schools seems to echo the influence of Lukacs's *History and Class Consciousness*. No one is suggesting that Mannheim followed Lukacs in his political commitment to Marxism, or that he directly challenged Weber's ethos for social science. What makes an interpretation of Mannheim's position in *Ideology and Utopia* so problematic is precisely the difficulty of determining whether there is some middle ground between these two perspectives.

In any event, Kettler, Meja, and Stehr rightly stress the significance of Mannheim's insistence that ideologies are cognitive structures—i.e., "productive of knowledge" (p. 61)—as a positive contribution to our understanding of political thought. This insight is still very far from gaining universal acceptance within social science. In part, this is due to the failure to appreciate the importance of Mannheim's observation that "ideology" is a distinctly historical phenomenon, as characteristic of Western society as the processes of rationalization or bureaucratization to which Weber drew attention. Even if this were granted, one would still have to demonstrate how very disparate areas of cultural consciousness are organized into an ideology, how the various elements that comprise this world view change differentially over time, and how the presence in society of competing world views relates to both these points. Such an undertaking requires a highly sophisticated concept of ideology, which certainly cannot be reduced to its functional role as a means of advancing the interests of a particular social group. In *Ideology and Utopia*, Mannheim argues that it is unlikely that such a concept of ideology will be formulated unless its historical-sociological status is first recognized, and that since no instrumentalist or positivistic methodology could produce this concept of ideology, the sociology of knowledge must employ both interpretive/phenomenological and causal/scientific methods in its treatment of ideology. How such a synthesis of methodologies is possible is a question whose legitimacy and thoughtful consideration has been set aside by the longstanding positivist/antipositivist debate within social science, but in the present period of the subsidence of the intensity of this debate, a rereading of *Ideology and Utopia* might prove to be a helpful first step toward supplying an answer to this question.

Like many others of his generation, Mannheim became a refugee from Nazism, settling in England. It seems fair to say that a noticeable decline in the intellectual quality of Mannheim's thought was the price of his adjustment to his new environment. Not that his writings on planning and democracy are devoid of valuable insights; on the contrary, *Man and Society* is, in its own way, a pioneering work, and still forms the starting point for much of the contemporary discussion of democratic planning. Nevertheless, Mannheim's suggestion of the psychoanalyst as the model for his social planner (Kettler et al., pp. 80–81, 147); his uncritical acceptance of the existence of a consensus on democratic values in England and America, and of the ability of parliamentary institutions to resolve all political conflicts in modern society (Kettler et al., pp. 27, 145; Loader, pp. 145, 154, 172); his easily misunderstood use of *manipulation* as a synonym for *planning* (Kettler et al., pp. 81–82; Loader, p. 144); and the missionary zeal he displayed in his quixotic attempts to reform the consciousness of his English contemporaries (Kettler et al., p. 130; Loader, pp. 174ff.) hardly exhibit the best qualities of his mind. As Kettler, Meja, and Stehr observe, Mannheim's failure to continue to ask critical questions concerning the foundations of his own position is the most disappointing feature of his post-1933 writings (p. 148).

Loader is less inclined to accept this view of Mannheim. In providing "a comprehensive interpretation of Mannheim's entire career," and in placing Mannheim in close intellectual proximity to Weber, Loader schematizes Mannheim's intellectual development into a "series of types, stages, and categories" (pp. 3–5, 186). *Intellectual Development* presents a "typological structure, which has been imposed upon the heterogeneity of Mannheim's thought" (Loader, p. 182); that is, the application to Mannheim's thought of Weberian ideal types as one-sided exaggerations forms part of the argument that Mannheim is, really, a Weberian thinker (Loader, pp. 179–80). Loader's argument is interesting, and does "cast a new light upon [Mannheim's] work" (p. 186). Nevertheless, I do not find his reading of *Ideology and Utopia* convincing. Loader's simpistic formula—that if Mannheim is not Lukacs, he must be Weber (pp. 101ff.)—is hardly helpful, and could easily be reversed. The constant use by Loader of the analytic and the valuative as sharply separable categories of sociological research draws upon a Weberian meaning that is, at times, more a hindrance than an aid to understanding Mannheim's approach. Readers of *Intellectual Development* should see pp. 178–89 for Loader's clarification of his procedure before they approach the text. Not only did Mannheim reject what he called Weber's "extreme nominalism," his reliance upon the individual as the unit of sociological analysis, and the sharpness of his separation of theory from evaluation in his conception of social science methodology, but as Kettler and colleagues observe, he "fundamentally alters the meaning" of the Weberian categories even when he employs them (p. 54).

Although my view of Mannheim is closer to that presented in *Karl Mannheim*, this should not be read as an attack on the plausibility of Loader's argument. A reading of Mannheim from the standpoint of Weber, especially when it is executed with a great deal of sensitivity, is a valuable contribution to the understanding of a thinker whose intellectual allegiances vacillate between Marx and Weber. Both these works are intelligent and sympathetic interpretations of Mannheim's thought, and like him, they raise important and yet unresolved questions concerning the nature of social science that deserve serious consideration.

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Marxism and Morality. By Steven Lukes. (New York: Oxford University Press, 1985. Pp. xiii + 163. $15.95.)

Steven Lukes's book breathes new life into a dying debate: that concerning the relationship between Marxism and morality. Others

(Cohen, Geras, Gilbert, Husami, Miller, Wood, and Young) have thoroughly explored available philosophical positions, but none from Lukes's practical perspective. Lukes bluntly asks whether Marxist theory accounts for the moral disasters of Marxist practice. He answers yes, and argues that socialists should sever some of their links to Marxist ethics, for Marxism is morally "disabled" and hence incapable of countering immoral measures pursued in its name. This gives the debate on Marxism and morality not only new vitality, but also new urgency.

Lukes begins by arguing that Marxists' views on morality are paradoxical: they attack morality as ideology, yet adopt a higher, human morality (chap. 2). Lukes resolves this paradox by showing how Marxists distinguish a morality of *Recht* from a morality of emancipation. They reject *Recht* (justice and rights) and the conditions requiring it (scarcity and conflict); emancipation involves the transcendence of both. Socialist society is characterized by cooperative abundance and harmonious interests, making *Recht* superfluous (chap. 3). Problems remain after this resolution, however. Lukes illuminates them by examining *Recht* (chap. 4) and emancipation (chap. 5) in depth. He argues that Marx (and his heirs) conceives *Recht* and the conditions necessitating it too narrowly. Rights need not only protect egoism, they can also defend individuals from arbitrary power. Similarly, justice involves more than the unequal treatment of unique individuals by a common standard; it provides general rules of fairness essential to social life. Since conflicting interests (though not class interests) and related questions regarding distribution, organization, and decision making postdate capitalism, Lukes argues that socialism requires *Recht.* Without a morality of *Recht,* Marxists cannot respond to injustices or to violations of rights; they cannot define impermissible means and inappropriate ends. In a final fascinating chapter, Lukes discusses debates over means and ends in the Bolshevik revolution (Luxemburg, Kautsky, Lukacs, Lenin), in the 1930s (Trotsky, Dewey), and in post-liberation France (Sartre, Camus, Merleau-Ponty, Koestler).

Lukes concludes that Marxists' exclusive morality of emancipation has two defects for guiding action. First, it generates illusions by refusing to specify, and claiming to foresee, the future socialist society. This precludes the clarification of ends by which to judge means, and hence consideration of alternate means. Second, these illusions further Marxists' moral blindness. Failure to clarify what socialism means in practice absolves revolutionaries of moral responsibility and denies them moral direction. It also prevents consideration of the relation between emancipation and *Recht.* Still, Lukes praises Marxists' vision of freedom, which is richer than the liberal view and reveals the ideological functions of *Recht.*

The insightful analysis and incisive questions throughout this book make it valuable for scholars and students of Marxism. As a self-proclaimed exercise in "socialist freethought," it chastens hostile critics of Marxism and challenges sympathetic ones. Lukes honestly confronts Marxists' moral disasters, arguing convincingly that they cannot afford to abandon *Recht.* But can they afford to adopt it? If socialist ends cannot be achieved by democratic means, are any other means justified by those ends? Lukes suggests not. Only political movements sensitive to *Recht* can create a socialist society worth having. But can they create it? And if so, how? In the end, Lukes only asks, "Can a theory of justice and of rights be developed which incorporates the insight and vision of Marxism's conception of freedom?" (p. 149). I wish he had answered, but that is perhaps unfair. He promises only a diagnosis, not a cure. He challenges others to answer, guided by the clarity and urgency of his questions.

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Marx's Critique of Political Economy: Intellectual Sources and Evolution, Vol. 1, *1844 to 1860;* Vol. 2, *1861 to 1863.* By Allen Oakley. (Boston: Routledge & Kegan Paul, 1984. Pp. 225. Vol. 1, $30.00; Pp. 302, Vol. 2, $32.50.)

In his recent, comprehensive, two-volume study, Allen Oakley provides a masterful examination of Marx's critique of political economy. The first volume devotes roughly equal attention to the works of the 1840s and 1850s; that is, to Marx's "early" and "middle" writings, from the *Paris Manuscripts* to the

Grundrisse and *Contributions to the Critique of Political Economy.* The second volume is devoted exclusively to that portion of Marx's critical manuscripts of the early 1860s published under the title *Theories of Surplus Value.*

Karl Marx's views and works on political economy prior to *Capital* have been a major source of commentary and controversy for several decades. Publication of Marx's 1844 *Paris Manuscripts* in the 1930s and its subsequent translation into English in the 1950s stimulated debate concerning the position of these manuscripts—notably Marx's critique of the phenomenon of alienation—in his life works. It was common in the 1950s and 1960s to postulate a tension or "break" between Marx's early writings of the 1840s and his more scientific analyses of capitalism's "law of motion," presented in *Capital.* The case for continuity was boosted by the English translation, in the 1970s, of Marx's massive unpublished notebooks of 1857-58, *Grundrisse.* This work, while continuing such early themes as alienation, linked them with Marx's emerging ideas on capital, value, and exploitation, subsequently published in more fully developed form as *Capital.*

On this controversy, Oakley takes a prudent middle ground. Marx's critical theory, he states, comprised a "complex mixture of continuity and discontinuity because of its organic and dialectical nature" (vol. 1, p. 2). Sifting out shifts in emphasis within elements of continuity can only be done by carefully studying Marx's life works as a whole. Reading *Capital* in isolation and out of bibliographic context provides a limited and misleading understanding of Marx's thought.

Oakley's chapter on Marx's 1844 *Manuscripts* usefully illustrates this view. While not neglecting alienation, the typical centerpiece of expositions of this work, Oakley deftly shows how this concept also provides (although in embryonic form and without the benefit of the mature Marx's theory of value) strategic elements of Marx's subsequently developed category of exploitation. Thus, Oakley shows that Marx's concept, if not his theoretical explanation, of exploitation has its intellectual roots in his early writings of the 1840s.

Another prominent debate surrounding Marx's early writings concerns the influence on Marx of his predecessors in classical political economy, notably Ricardo. One familiar tradition in Marx scholarship emphasizes the impact on Marx of the classical political economists, and the continuity between them. Another, more recent, school of thought challenges this interpretation, and focuses on Marx's special, essentially non-Ricardian contribution.

On this issue, also, Oakley charts a judicious course between the extremes of considering Marx a mere "minor post-Ricardian" and asserting a fundamental dichotomy between classical and Marxian perspectives. Marx relied "very heavily" on classical political economy in constructing his own critical theory of capitalism, but "he went to great lengths in order to differentiate and distance his critical theory from the work of his antecedents" (vol. 1, pp. ix-x). As Oakley explains, this differentiation proceeded through two main stages. In Marx's writings of the 1840s, he essentially accepted classical political economy as a "faithful abstract reflection" of the actual capitalist society (vol. 1, p. 131). Increasingly, however, Marx came to the conclusion that classical political economy failed to appreciate fully the tensions and contradictions behind the apparent liberty and equality of capitalism's market exchange processes and regime of free contract. Consequently, he shifted from a critique of capitalism via classical political economy to a critique of the limitations of classical political economy as well. This, in turn, required formulation of his own critical alternative analysis of capitalism, culminating in *Grundrisse.*

Although Oakley's careful and insightful study sheds valuable light on these and other debates, his stated intent is the more modest one of expositional clarity. That he achieves more than this in the process is a pleasant bonus. He certainly succeeds in his stated task of clear exposition of the intellectual influences on and evolution of the "critico-theoretical core" of Marx's thought.

Oakley's equally painstaking explication (in vol. 2) of the manuscripts subsequently published as *Theories of Surplus Value* is especially valuable because these manuscripts have been relatively neglected by Marx scholars, and because they constitute Marx's clearest and fullest attempt to differentiate and distance his work from that of his predecessors. As in his earlier writings, Marx thrived on critical analysis and evaluation of others' ideas. Through critique of his predecessors' views of surplus value, Marx engaged in "self-clarification," and thereby advanced the sophistication "of his own critical theory of how capitalism functioned" (vol. 2, p. 6). Thus, *Theories of Surplus Value* is both more and less than ruminations on the history of political economy in the traditional sense: more, because he used critique as a means to advance his own thinking; less, because the scope of his manuscripts was narrowly proscribed by his own particular interest—surplus value—and his own particular criterion for judging the quality of his antecedents' works, namely, the labor theory of value.

Apart from the introductory and concluding chapters, Oakley organizes his discussion around two overarching themes: the key issue of value and distribution theory, and the high theme of capitalism's "law of motion."

As for the first of these two topics, the fundamental dividing line, of course, is Marx's assessment of the contributions of David Ricardo, whom Marx clearly regarded as both the classic representative and apogee of classical political economy. All other views of exchange value and distribution are either "pre-" or "post-Ricardian."

Oakley provides both order and insight in his summaries of Marx's interpretations of surplus value in the writings of his precedessors. From Marx's perspective, mercantilist writers explained economic surpluses (of production above necessary consumption of the working population) as derived purely from exchange; that is, from the sale of commodities above their values (vol. 2, p. 33). Physiocrats such as Quesnay located—quite properly in Marx's view—the origin of surpluses in production rather than exchange, but restricted their argument to the physical realm in general and to agriculture in particular (vol. 2, pp. 37–38). Adam Smith sharpened the analysis by generalizing from the creation of surplus to production as a whole, attributing a special role to labor, and interpreting surplus in terms of value, rather than merely in physical terms. However, he muddied the waters by presenting a conflicting view, in which "natural prices" are themselves nothing more than the summation of wages, rents, and profits at their "natural rates" (vol. 2, chap. 3). According to Marx, Ricardo's great achievement (relative to Smith) was to penetrate to capitalism's "inner," more fundamental, relationships through a labor-embodied theory of value. Yet even Ricardo failed to articulate fully the relationships between value and distribution, by not clearly differentiating between fixed and variable capital, surpluses and profits (and thereby between the rate of surplus value and the rate of profits), and labor and "labor power," and by his ingenious—but from Marx's standpoint digressionary—focus on land rent (vol. 2, chaps. 4–5). After Ricardo, Marx's assessment of the quality of political economy (and his patience) decreased. Still, Oakley helpfully guides his readers through four chapters of commentary on Ricardo's seminal work.

In a final section, Oakley turns to more "dynamic" aspects of *Theories of Surplus Value.* These chapters also provide him an opportunity to elucidate Marx's own emerging analyses of capitalism's "law of motion." Rather quickly (too much so for this reviewer's taste), Oakley traverses reproduction schemes —both his own and Quesnay's—(chap. 10), capital accumulation and the falling rate of profit (chap. 11), and crises (chap. 12). The last of these three discussions, although abbreviated, is the most interesting. Here, Oakley comments insightfully, in rapid succession, on Marx's critique of Say's Law (that is, the proposition that the production of output generates income, which thereby constitutes sufficient demand for the output, thus circumventing crises and depressions); partial and general overproduction; Ricardo, James and John Stuart Mill, Malthus, and Sismondi; and the crucial distinction between the possibility and causation of crises.

Oakley's study is an important, needed, and unique contribution to Marx scholarship. He creates order and clarity out of three massive volumes of Marx's posthumously published manuscripts. He shows how Marx differentiated his own work from that of his predecessors, clarified and advanced his own thinking, and thereby prepared the way for embarking on the critical investigations he drafted in *Grundrisse* and subsequently published in *Capital.* Although obviously sympathetic to Marx's project, Oakley carefully notes limitations and inconsistencies in the argument. The final section, on dynamic elements in Marx's thought, could easily have been doubled in size, and suffers by comparison with the fuller and more leisurely examination of theories of value and distribution. All in all, however, Oakley's explication of Marx's *Theories of Surplus Value* is a valuable contribution and a most useful guide to a neglected and strategic part of Marx's voluminous writings.

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Disorganized Capitalism: Contemporary Transformations of Work and Politics. By Claus Offe. Edited by John Keane. (Cambridge: The MIT Press, 1985. Pp. 366. $12.50.)

Claus Offe has established himself as one of the most important contemporary thinkers associated with critical theory and the Frankfurt School. This volume contains reprints, some newly revised, of ten of Offe's recent essays on the contradictions of the contemporary capitalist states. Despite their various subject matters, each of these essays poses a challenge to the notion of "organized capitalism." This idea, first propounded by Rudolf Hilferding in 1910 and developed in the work of German democratic-socialist theorists in the 1920s, notes the passing of classical market capitalism and its replacement with "formally organized collectivities of economic action (corporate firms, cartels) and interest representation (trade unions, business associations)" (p. 6). Instead of resolving the problems of classical capitalism, Offe analyzes the inherent tensions and contradictions that plague these corporativist-collectivist structures of late capitalism.

The first five essays ("The Political Economy of the Labour Market," "The Future of the Labour Market," "Three Perspectives on the Problem of Unemployment," "The Growth of the Service Sector," and "Work: The Key Sociological Category?") focus on recent developments in the sociopolitical status of labor in late capitalism. Throughout these essays, Offe notes the problems of treating labor in terms of "the labour market." This notion of the market misperceives labor as just one commodity among many others. The second five essays ("Interest Diversity and Trade Union Unity," "Two Logics of Collective Action," "The Attribution of Public Status to Interest Groups," "Legitimation Through Majority Rule?" and "The Divergent Rationalities of Administrative Action") explore the failures and inadequacies of the mediating links between social power and political authority. In the most general terms, the politicization of social and economic structures does not resolve the problems of classical capitalism, but merely transfers them to the institutions of the state.

Like most of his theoretical work, Offe's *Disorganized Capitalism* is poised between Marxist and Weberian perspectives. The emphasis on contradictions and inherent tensions clearly reflects the Marxist tradition of the critique of existing theories and practices. Furthermore, Offe attests that his work has an explicitly political, delegitimizing goal:

> If these assumptions [about the integrating tendencies of welfare-capitalist democracies] become implausible when exposed to a reality test, this will in turn contribute, . . . to the disorganization of not only our mode of thinking about modern capitalism but also, and consequently, to the political disorganization of the patterns of organization of welfare-capitalist democracies themselves. (p. 9)

However, no facile conclusions about the imminent or automatic collapse of late capitalism should be drawn from Offe's critique. Following Weber, Offe contends that the contradictions and inherent tensions of late capitalism may continue to be managed, if not resolved, within the parameters of the existing capitalist state.

Offe's writing style is forbidding. These essays are highly abstract and complex theoretical exercises. The relatively few empirical-historical references provide precious moments of relief from this theoretical assault. Yet Offe's work continues to take its proper place in the first rank of research on the modern state. A "Chronological Bibliography of Claus Offe's Work" is appended to this volume.

JOHN BOKINA

Pan American University

Giving Desert Its Due: Social Justice and Legal Theory. By Wojciech Sadurski. (Dordrecht, Holland: D. Reidel Publishing Company, 1985. Pp. xvii + 329. $47.00.)

It is impossible in this brief space to give this deserving book its due. Ranging over topics such as legal justice, ethical justification, the proper distribution of wealth, respect for human needs, preferential treatment, and the ethical basis of punishment, Sadurski attempts to demonstrate that essentially one principle of justice can and should be applied in the numerous settings in which questions of justice arise. The unifying principle is that social arrangements should aim toward an "equilibrium" of the benefits and burdens experienced by individuals as they strive to implement their life plans. Sadurski's idea is not to equate benefit and burden, as is done in interpretations of punishment as a restoration of the statue quo ante. Rather, he wants, as he often puts it, for society to "balance" benefits and burdens so that one truly gets what one deserves. This emphasis on receiving the proper rewards for one's efforts introduces a dynamic element to this conception that, perhaps, justifies the additional connotations of the notion of equilibrium.

The substantive breadth of this book is complemented by a certain narrowness in its definition of justice. For Sadurski, justice basically concerns distributive issues only. For example, he does not consider the aggregate amount of freedom in a society to be, in itself, an issue of justice. On the positive side, this narrowness promotes conceptual clarity and the consistent application of a coherent conception of justice across its many spheres (to borrow Michael Walzer's phrase). On the negative side, one is left in the dark, or at least the twilight, about how to trade off justice with, say, freedom or utility.

There are some additional areas that need clarification. Sadurski, for example, proposes that balancing should occur between the social benefits a person produces and the burdens experienced in producing them. Specifically, people should not be rewarded for their personal attributes, but only for the burdens they encounter in using them. Yet the notion of burdens employed by Sadurski is ambivalent. When basic human needs go unsatisfied, this counts as a burden even though it is only counterfactual—that is, of the form "so-and-so could have done *X* if he or she were not in need of *Y*." No actual effort need be expended. Once basic human needs are satisfied, however, burdens, as I understand Sadurski, are those encountered through actual effort (p. 116). In this labor theory of desert, advantages due to undeserved resources are negated, but potential that remains untested due to nonbasic burdens is apparently undeserving. Saturski's heart is clearly with the latter restriction on the notion of burden, but he recognizes a requirement to satisfy basic needs so as to put individuals in a position to produce socially valued benefits. It is not enough, though, to postulate that deprivations of the opportunity to produce would simply be unjust. Injustice, after all, is what we are trying to define.

It is interesting in this respect that Sadurski argues that the allocation of jobs need not conform to the basic principle of justice (pp. 153-56). Jobs should be awarded on the basis of merit and not actual past sacrifice. Here we see a very active and narrowly construed notion of burden and a curiously uncritical acceptance of the ideas by which we have supposedly chosen to allocate jobs (this from an advocate of preferential treatment). He also suggests that a job is not really an unalloyed benefit, but encompasses burdens as well. It is arguable, however, that in a free market, a job is more of a benefit than a burden.

With regard to the general notion of benefits, there is a possible incompleteness in this theory of justice, even taken on its own terms. In rewarding people for their socially beneficial efforts, we need to rank different kinds of effort and compare then with standardized measures of reward (p. 143). Those very comparisons are, in turn, typically institutionalized through markets and other social or political arrangements (see pp. 127-28). Arguably, they must be institutionalized in some sense to become socially meaningful. Can Sadurski rest with his admonition to balance burdens and benefits, or must he assess the alternative institutional mechanisms in which these elements are first defined? Is he thus led back to the idea that the basic structure of society is at least part of the subject of justice? This book provokes and merits questions such as these.

ROBERT GRAFSTEIN

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Maurice Merleau-Ponty: Between Phenomenology and Structuralism. By James Schmidt. (New York: St. Martin's Press, 1985. Pp. x + 214. $30.00, cloth; $10.95, paper.)

The world of political philosophy today is divided into a number of subsets (at times appearing to be armed camps). When a reader sitting in one subset meets a book from another, the result is often bewilderment—perhaps even hostility—followed, one hopes, by an attempt to understand and come to terms with a different vocabulary and unfamiliar concepts before returning to the comfort of familiar surroundings. This is to say, a specialist in contemporary continental—specifically French—political philosophy, which I am not, would read the book under review differently than I did.

For the nonspecialist who is somewhat familiar with the vocabulary and concepts involved but does not live with them every day, this is a valuable book. James Schmidt takes the reader step-by-step through Maurice Merleau-Ponty's intellectual life, from his earliest writings to his last. His intellectual evolution and his periodic revolutions in stance are recounted fully and fairly. Anyone wanting a reasonably sophisticated introduction to Merleau-Ponty will find one here.

The problem, which appears at times to bother Schmidt, is whether anyone but a specialist should care. The first paragraph of the book gives a number of very good reasons why we should care:

> By the time of his death, Merleau-Ponty was regarded as 'the greatest of French phenomenologists'; yet his last writings questioned the very coherence of Husserl's project. He built a case for Marx's philosophy of history which is among the most subtle in the Marxian tradition; and he produced one of most powerful and merciless critiques Marxism has ever received. He was the first French philosopher to appreciate the importance of Saussure's linguistics; but even his admirers admit that the things he purported to find in Saussure are simply not there to be found. He was the author of a corpus of works whose relevance for contemporary social theory has been matched only by the striking indifference which they all too often have received. (p. 1)

This is a strong statement of significance, yet the Merleau-Ponty that emerges at the end of the book was at best a transitional figure (there is nothing wrong with that) and probably a quite minor one. What happened?

First, to avoid any overstatement of the case, being a minor figure worth attention in a league including Sartre, Husserl, and Saussure is no small achievement, and Merleau-Ponty was attempting to meet on equal terms thinkers of the caliber of Descartes and Marx. In this company it is difficult to avoid appearing minor.

Second, Schmidt is intensely aware of and bothered by the fact that Merleau-Ponty blatently misrepresented his sources, particularly Saussure. Schmidt rather harps on this failure to observe the canons of scholarship, but how important is it? As Schmidt points out, this fact has been rather ignored by Merleau-Ponty scholars; therefore, it is essential that Schmidt warn the reader (and there is undoubtedly a good article waiting to be written on the subject), but once we are warned, it is not really all that important. Aristotle misrepresented Plato; it is, unfortunately, part of the tradition of philosophy to progress by climbing on the misrepresented shoulders of past thinkers.

Third, Merleau-Ponty took on so many of the greats on so many grounds that it is at times difficult to follow him. This Schmidt does admirably, but Merleau-Ponty's changing concerns convey the feeling that he never really finished anything. The sheer breadth of Merleau-Ponty's interests gives an impression that he lacks depth. Schmidt argues against this impression, but in a book of just over 200 pages in which the text occupies only 167 pages, he simply doesn't have the space to do so successfully.

The appearance given by both Merleau-Ponty and Schmidt of a constantly dissatisfied intelligence periodically grasping at straws in an effort to find a stable point should not obscure the fact that the problems that bothered Merleau-Ponty were very real problems in the positions he criticized. Merleau-Ponty found what he thought to be fundamental flaws in phenomenology, Marxism, and structuralism, and forced their exponents to change or abandon certain arguments.

Besides, perhaps a constantly dissatisfied intelligence is the most appropriate model for the last quarter of the twentieth century. As scholars, shouldn't we be more concerned with the thinker who asks the fundamental questions rather than with those who think they have found the definitive answers? While

Schmidt doesn't pose the problem this way, he concludes with a metaphor intended to emphasize that change is the only constant in philosophy, and suggests that Merleau-Ponty will have contributed to the next step. Be that as it may, anyone interested in the intellectual history of the twentieth century will find *Maurice Merleau-Ponty* worth exploring.

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The Challenge of Social Control: Citizenship and Institution Building in Modern Society. Essays in Honor of Morris Janowitz. Edited by Gerald D. Suttles and Mayer N. Zald. (Norwood, NJ: Ablex Publishing Corporation, 1985. Pp. vi + 250. $34.50.)

This is a collection of essays that were delivered at a conference honoring Morris Janowitz at the University of Chicago in 1982. The essays focus on topics that Janowitz has researched and written about during his illustrious career: the fragmentation of political institutions, the welfare state, social movements, the all-volunteer army, the legitimization of political authority, and urban institutions. Janowitz is the best known political sociologist to embrace institutional analysis, and the essays of these colleagues and students all have the master's mark on them.

The volume is divided into five parts. Part 1 contains an overview of Janowitz's ideas and career by Gerald Suttles and an essay by Don Levine on sociological theory. Levine's article is relevant in light of the Heritage of Sociology series published by the University of Chicago Press and edited by Morris Janowitz. Part 2 presents essays by Thomas S. Smith and Allan Silver. Smith addresses the issue of how individuals react to the absence of an integrated cultural order and of leadership in work groups. Silver describes the role that trust plays in the economic, social, and political arenas. Both essays extend Janowitz's work on social control. For Janowitz, *social control* is the capacity of a social system to govern itself according to a set of higher social or moral principles. Governments are viewed as proactive forces in society in which these values are crystallized and given concrete expression. The problem of generating trust in governments is not something that Janowitz discusses at length, and Silver highlights some issues that all polities face. For example, it is relatively easy for trust to emerge in face-to-face recurring interaction, but it is more difficult to establish a trusting relationship between citizens and impersonal bureaucracies. Silver also focuses on an issue that Janowitz has only touched upon: What happens when values aren't articulated or leaders don't lead? Silver suggests that people withdraw into personal dyads for meaning, abandoning the public forum.

The third and fourth parts of the volume are of more interest to political scientists. Heinz Eulau and Vera McCluggage address one of Janowitz's most persistent concerns: the fragmentation of governmental institutions. In particular, they look at congressional committees. This essay highlights the complex processes that produce the proliferation—not fragmentation—of committees. Although the article wallows in functional imagery, it presents a sound critique of Janowitz's lament. Dwaine Marvick addresses the quality of electoral politics, another problem that has preoccupied Janowitz. The article is brief and suggests that elections can be scored on the basis of how manipulative activists were and how attentive citizens were. The essay by Roberta Garner and Mayer N. Zald is an extended theoretical treatise on the different dimensions of social movement sectors. It also looks at systematic constraints that can explain variation in these dimensions across societies. The authors compare the social movement sectors of the U.S. and Italy to illustrate their points. Clearly the article is more Zald than Janowitz, and will be more interesting to students of social movements than to the Janowitz discipleship.

Kirstin Gronbjerg, Charles Moskos, and David Segel look at the welfare state and military institutions. I view these three articles in a similar light because they all have a component of action or public policy. While he may object to the label "applied sociologist," Janowitz is clearly a man of his times. Moskos and Segel both address issues related to the all-volunteer army and national service. Moskos outlines proposals to implement the all-volunteer force; Segel reviews the theoretical and historical conflict between sociological and economic conceptions of what the all-volunteer army should be. Gronbjerg traces the growth of the welfare state and the institutionalization of a mass-society ideology of citizenship rights. She then wrestles with the implications of a long time Janowitz truism—that the welfare state and the extension of citizenship rights are only possible in a growth economy. With tax revolts and Reagan's effort to bolster the military establishment only complicating matters, she speculates on what may follow: disenfranchisement, citizen withdrawal, increased inequalities, and weakened governmental legitimacy. Also—in true Janowitz style—she suggests ways out of the dilemma, ranging from reductions in transfer payments and loans to the middle class to the institutionalization of a voluntary national service.

The final section of the volume focuses on urban structure and also has a policy focus. After reviewing where people and jobs are moving in the contemporary metro setting, John Kasarda suggests that planners should cease trying to reverse powerful trends that are primarily market driven, and develop policies that are consistent with these trends. Jack Ladinsky and Charles Susmilch, William Kornblum, and Albert Hunter move to the neighborhood level and focus on institution building; unfortunately, none of the efforts cited are very encouraging. Ladinsky and Susmilch find that consumers do not use community-based institutions to resolve their difficulties with faulty products and services; Kornblum's case study of a New York City high school depicts utter chaos and despair; and Hunter's theoretical piece on crime control highlights the inability of various levels of social life in the city to coordinate their efforts and interests.

As a whole, the volume is successful as a testimony to Morris Janowitz—to how he inspires others to explore political, urban, and social concerns for their own sakes and come up with ideas for social change. Janowitz is not a grand theorist, but rather does his work in the middle range, identifying phenomena or processes that others ignore and exploring how these interface with the rest of society. Janowitz is an active participant in implementing change that he believes is necessary—especially in military and urban affairs—but he is not a revolutionary. He sees change as an incremental process. All the articles reflect this orientation and, to my mind, should serve as models for future scholars and policy makers alike.

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A Rationalist Methodology for the Social Sciences. By David Sylvan and Barry Glassner. (Oxford: Basil Blackwell Publisher, 1985. Pp. vi + 188. $29.95.)

The problem addressed is how to make rationalist theories better, that is, more eloquent and orderly: "Are tools available for more rigorous and systematic inquiry in the tradition of these rationalist theorists?" (p. 9).

The answer is a tentative yes. A case is made using Simmel's conflict-cohesion hypothesis (external conflict-internal group integration). Other major examples are French structuralism (Levi-Strauss) and new social realism (Giddens). Simmel's hypothesis is formalized and analyzed with a logic designed in a computer program (LOGLISP).

The world of social analysts is divided into rationalists and empiricists:

> Rationalists seek to make coherent sense of the social world rather than to manipulate empirical phenomena through experimental theories and other methods. In rationalist theories explanation aims at logical possibilities, not statistical probabilities; the concern is with combination, not causality. (p. 1)

Another way of expressing this distinction is in terms of macrotheorists and micro-observers.

Rational theorists, however, do not have a "methodology." Without something like the empiricists have, it is difficult to sustain a profession—we may degenerate into a collection of associated individuals, each relying on our personal insights and languages. The authors offer formal modeling as a methodology that, they argue, is at least not incompatible with the thinking of rationalist theories. Something more than discussion can be done. The reasons given for creating a methodology for European structuralists are lack of clarity and consistency.

There are many fine points scattered throughout the book, including criticisms of major controversial thinkers such as Althusser and Poulantzas. One is that existing languages, of which logics are one type, are not suitable for the social world made up of contours, bumps, and contradictions. The development of catastrophe theory in the early 1970s awaits convincing examples in the social sciences. The authors' arguments open up another set of possibilities for the 1980s.

The predicament of the social sciences is that most of its empirical research has produced few noticeable theoretical yields; yet its theoretical creations do not translate well into coherent statements and research protocols. Some 50 years ago, F. A. von Hayek assessed the European (German) historical school's pursuit of applying the methods of the natural sciences as one reason for the collapse of economic theory. He predicted that such an effort "is bound to degenerate into mere record and description and a total skepticism concerning any laws at all" (F. A. von Hayek, ed., *Collectivist Economic Planning*. London: Routledge, 1935, p. 9).

The main road to theory is thinking, rather than organizing to build theory upon observations. If formal modeling will help thinking—and I believe it can—then perhaps the best way is to put both capacities into the heads of the creators of theories. This book, however, accepts the largely American tradition that methodologies constitute a separable "field," whether in observation or speculation.

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Behavioral Decision Making. Edited by George Wright. (New York: Plenum Press, 1985. Pp. 407. $47.50.)

The subject of decision making is parceled out among so many disciplines and subfields that nobody can keep up with all the scholarship on the subject. Political scientists, economists, management scientists, sociologists, psychologists—all have staked claims to a piece of the action. In this book, as the code word *behavioral* indicates, we get an opportunity to see what psychologists and decision analysts have been up to recently in their empirical research on the ways in which people make choices. The papers in the collection are concerned primarily with understanding how and why individuals depart from the principles embodied in "expected utility" models of decision making—i.e., why people do not do the things that decision analysis shows they should do—and with exploring whether and how decision aids can improve decision outcomes. The authors review a good deal of recent research on decision analysis in fields as various as architectural design and insurance underwriting.

The sections on individual and small-group decision making are the strength of the book, and illustrate the interesting questions that psychologists address and the often ingenious research methods that they use to study them. They test the effects of such variables as task complexity, time pressure, and the way in which a problem is framed. The paper by Amos Tversky and Daniel Kahneman gives a number of illustrations of how small differences in the conceptualization of a problem lead to significant differences in choices.

In the section on organizational decision making, the editor turns the pages over to two sociologists, who create an island in the book quite different from anything that comes before or after. Roger King draws on political science and sociology to address issues of power—what it is and how to measure it. His chapter reviews the difficulties of conceptualizing power in the light of "nondecisions" and "false consciousness." The paper is a sophisticated assay of theories of power, but since it barely alludes to decision making, it seems to have strayed into the wrong company. On the other hand, the excellent paper by Weeks and Whimster confronts the subject of behavioral decision making directly. They present a devastating critique, in fact several critiques, of the rational and individualistic assumptions that underlie it. They cite Tom Burns's conceptualization of an organization as an interaction of three different systems—a working organization, a political system, and a career structure. When individuals calculate the utilities of organizational choices, their referent can be political influence or career prospects just as well as the goals of the working organization. Weeks and Whimster also discuss Habermas's critique of rationality and his alternative formulation of communicative rationality. This chapter, although in contention with the papers around it, fits the book well and is likely to be of interest to political scientists.

The latter part of the book analyzes the capabilities of decision theory. Several papers have something to say about the role of decision aids in improving decision making. In a section on judgmental forecasting, a paper by Hillel J. Einhorn and Robin M. Hogarth discusses the ways in which causal judgments affect the use of formal statistical models. An appealing paper by Allan H. Murphy and Barbara G. Brown assesses the accuracy of objective and subjective methods of weather forecasting. Objective methods are those that use numerical data and/or statistical models; subjective forecasts depend on the judgment of meteorologists. The paper concludes, you will be happy to hear, that both objective and subjective forecasts are generally accurate and have improved in accuracy over the period from 1971 to 1982. Subjective forecasts tend to be slightly better for predicting precipitation and temperature in the near term; objective forecasts are better for predicting the amount of clouds.

The final section of the book looks for areas in which decision theory may have impact in the future. Jack Dowie's paper suggests that university education is an arena in which decision theory approaches might be applied—for example, by allowing students to answer multiple choice tests probabilistically. That is, students would be asked not only to give an answer, but also to indicate their level of certainty that the answer is right.

Of the 21 contributors to the book, 11 are British, including Wright himself, who is with the Decision Analysis Group, Department of Psychology, City of London Polytechnic. This gives a pleasant flavor to the writing and to some of the illustrative examples, although most of the literature cited is American. Wright is proud of the fact that 14 of the 17 papers were commissioned expressly for the volume and appear here for the first time. However, even commissioning does not necessarily produce a volume that has a coherent structure. The sections of the book sometimes appear to be arbitrary clusters of papers rather than a set of logically related ideas. Furthermore, the book lacks an index.

If you want to learn about recent work in decision theory approaches to decision making, this is an engaging and particularly wide-ranging set of articles. (The editor suggests that a novice might want to begin with a more elementary treatise.) If you come to the book looking for insights that will help explain the decision-making behavior of legislators or the outcome of public policy debates, it will take creative reading and leaps of inference to extract them.

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AMERICAN POLITICS

Setting Municipal Priorities, 1986. Edited by Charles Brecher and Raymond D. Horton. (New York and London: New York University Press, 1985. Pp. xxiii + 476. $60.00, cloth; $30.00, paper.)

Union Power and New York: Victor Gotbaum and District Council 37. By Jewel Bellush and Bernard Bellush. (New York: Praeger Publishers, 1984. Pp. xxii + 474. $33.75.)

Setting Municipal Priorities, 1986 is more a government yearbook than a scholarly monograph. It is the seventh in a series dedicated to surveying New York City's administrative operations, edited by Charles Brecher of New York University's Graduate School of Public Administration and Raymond Horton of Columbia's Graduate School of Business. The contributors are largely professors from the aforementioned universities and policy analysts from nonprofit research foundations. Each chapter deals with a specific public policy area, with particular emphasis on the delivery of services, but with coverage of the local economic condition and outlook as well as revenue and spending policies. The authors largely employ a common format, and the chapters are of uniformly high quality, even conceding the difficulties associated with multiple authorship. The use of statistical data and their integration into the discussion is exemplary, and the "future policy options" proposed are usually thoughtful and fair minded. It is curious that such a work succeeded in incurring the wrath of Mayor Edward Koch, who has questioned the scholarly objectivity of the research (*New York Times*, November 9, 1985).

Although each policy area reviewed comes with recommendations for programmatic improvements, the contributors are stronger in describing problems than in proposing solutions. The analysis is vastly informative, replete with statistical tables and abounding in basic information presented in an unbiased fashion—analytical, but unfortunately not very political. Too many of the recommendations are managerial and technical, without addressing themselves to the political realities underlying policy choices. There is no Ed Koch, Victor Gotbaum, Al Shanker, or Bobby Wagner in *Setting Municipal Priorities*; nor is there any overall set of priorities for the city government to pursue. On the other hand, most of the specific city services are examined with great care. For example, John Kaiser's chapter on sanitation documents the dramatic productivity improvements that have been realized in a notoriously demoralized department, and the chapter on criminal justice by Esther Fuchs and John Palmer Smith thoughtfully analyzes commonplaces about crime, criminals, and courts with good use of data and a strong grasp of the literature. In sum, the quality of the policy research in *Setting Municipal Priorities* is very high. If one wants to get a firm grasp of the background and issues of the policy problems confronting New York City, this is the place to start.

Union Power and New York, essentially a life and times of Victor Gotbaum and of District Council 37 (the AFSCME local in New York City), is a very different sort of work: qualitative, personal, sometimes emotional, and definitely not disengaged. Indeed, it is essentially a memoir of the union written in a tone of easy familiarity with its leaders, demonstrating extensive knowledge of its history, and exhibiting clear sympathy for its programs and goals. For the Bellushs, this book is clearly a labor of love, and just as clearly reveals a depth of research in the archival materials that is quite impressive. On the other hand, the authors sometimes indulge themselves in value-laden references that give "thanks for Gotbaum's heroic leadership" (p. 99), or cite the "limitless devotion to the rank and file" of DC 37 Associate Director Lillian Roberts (p. 211). This is not to imply that *Union Power and New York* is biased or lacking in overall objectivity; it would, however, have benefited from a less critical approach and greater analytical rigor.

It is frequently noted that DC 37 has been in the forefront of both public and private sector unions in providing economic and educational benefits for its members and in supporting important social welfare legislation benefiting the public at large. These are accomplishments with which the authors are in agreement, and what the Professors Bellush have done very well is to give a detaied account of the rise of Victor Gotbaum as a labor leader and DC 37 as a public-employee labor union. If the book is short on hypotheses, it is replete with information about the union's management and political activities. It also conveys a vivid sense of the passionate commitment to the principles of social egalitarianism and ideological liberalism that has been such an important part of the style of New York City politics.

As the leader of the largest public-employee local in the country, Victor Gotbaum has exerted a major influence on the politics of both New York City and the state of New York. Certainly, Gotbaum's most visible and far-reaching accomplishment was the role that he played in forging the alliance of unions, banks, businesses, and city and state governments that kept the City from lurching over the brink of bankruptcy during the period 1975–77. This herculean task would have been an extraordinary feat of leadership for someone from a secure political base. Gotbaum, however, had to support budgetary austerity measures that diminished his membership's benefits, even while he insisted that the entire burden was not to be borne by public employees and the public's services. The Bellushs sum up the dual orientation that has characterized DC 37 as follows:

> Philosophically, Gotbaum gave the Council an independent, progressive orientation on a wide

> range of issues. District Council 37 has defended the rights of the poor and aged, supported the ecological movement, and spoken out forcefully in the struggle for civil rights and human dignity. (p. 453)

At the same time, Gotbaum also knew "that he had to fulfill his major functions as a union leader which was to negotiate good contracts on the fundamental day-to-day concerns of the members" (p. 454).

Whatever one may think of DC 37's ideology and politics, there is no question that *Union Power and New York* conveys a sense of the peculiarities of the City's politics that is missing in the more detached *Setting Municipal Priorities.* Both are to be recommended for different reasons to the reader who wants to know what makes New York unique among American cities.

KEVIN V. MULCAHY

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Farm Policy: The Politics of Soil, Surpluses and Subsidies. By Nancy A. Blanpied. (Washington, D.C.: Congressional Quarterly Inc., 1984. Pp. ix + 179. $9.95, paper.)

Made in Washington: Food Policy and the Political Expedient. By Clarence D. Palmby. (Danville, IL: The Interstate Printers and Publishers, 1985. Pp. xi + 226. $14.95.)

Alternative Agriculture and Food Policies and the 1985 Farm Bill. Edited by Gordon C. Rausser and Kenneth R. Farrell. (Berkeley: Giannini Foundation of Agricultural Economics, University of California—Berkeley and the National Center for Food and Agricultural Policy, Resources for the Future, 1984. Pp. xix + 425. $18.00.)

U.S. Agricultural Policy: The 1985 Farm Legislation. Edited by Bruce L. Gardner. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1985. Pp. viii + 385. $12.95, paper.)

The plight of the farmer has been a recurrent theme throughout all of American history. There have been periods of farmer prosperity, to be sure, and not the least of these was during the 1970s. On-farm income moved up to levels which made the farmer about equal in economic status with urbanites. Federal costs for agricultural commodity programs (excluding, of course, food stamps, research, etc.) had shrivelled in that period to an almost inconsequential $600 million. Then came "the terrible 80s," and once again the media were overwhelmed with truly heartbreaking stories of farm foreclosures and even murder and suicide. Moreover, public costs of agricultural programs rocketed to frightening heights—an estimated $24.6 billion by 1986—and all the while the farm crisis was exacerbated.

Congress hardly needed to be confronted with another crisis. Indeed, the farm economy during the 1970s was a major respite from the nation's economic ills. The value of farm exports almost exploded from some $6.7 billion in 1970 to almost $32 billion by 1979, and this remarkable increase was certainly helpful in mitigating the adverse effects of the national balance-of-trade deficit. However, for numerous reasons which cannot be examined here (high interest rates, an almost explosive rate of inflation followed by an overvalued dollar, and severe budget deficits) the 1980s brought the farm issue back to a prominent place on the congressional agenda. Moreover, the 1981 Farm Act expired in 1985, and to revert to the legislation of 1949 would have burdened Congress with even more onerous budget costs, and in the long-run would have seriously aggravated an already depressed farm economy.

Congress had to act, and eventually—by December 1985—the Food Security Act of 1985 was passed. To a major extent (although somewhat less so with the study by Clarence Palmby), all of these books were scholarly efforts to influence this bill; that is, they sought to affect who was to get what, at what costs, and with what effects on the U.S. Treasury, the farmer, agribusiness interests, and the rest of us.

Farm Policy, Nancy A. Blanpied's study for the Congressional Quarterly, can be recommended for use by those teaching introductory undergraduate courses in American government who wish to have their students acquire a general understanding of U.S. food and agricultural politics and policies. The presentation is heavily descriptive, as it should be at that level of instruction. The discussion of "agricultural issues" (part 1) is presented in an even-handed and sufficiently comprehensive manner. Particular praise is due the chapters on soil erosion, farm migrant labor, agricultural research, and farm pesticides. One might, however, have expected a chapter on nutrition and food-based welfare programs. Part 2 deals with the development of U.S. agricultural and food policies since the end of World War II, and these chapters are also handled in a readable, competent form and style, although the bibliographic references are less than impressive.

Made in Washington, by Clarence Palmby, is essentially an autobiography, interlaced with a fair amount of personal ideology and policy proposals. Palmby has had a moderately distinguished public career in American agriculture—executive vice-president of the U.S. Feed Grains Council from 1961 to 1969, assistant U.S. secretary of agriculture in the early 1970s, and vice-president of public affairs for the Continental Grain Company thereafter for nearly a decade. We are indebted to him. Political science literature needs more first-hand, candid, analytical recounting from public officials who have also served prominently in quasi-public and agribusiness companies. His account of the Russian wheat sale in 1972, in which he was "bruised" by "false and unfounded" interpretations (pp. 141–42), is an important and largely persuasive addition to our scanty knowledge of what happened. Also, Palmby's description and defense of the origin and role of the U.S. Feed Grains Council in building U.S. farm export markets needs to be understood by students of U.S. trade policy. His discussion of past international wheat agreements and aborted efforts to negotiate a more comprehensive wheat agreement in the early 1970s should be useful to those who will be studying the General Agreement on Tariffs and Trade (GATT) negotiations—both past and (likely) of the near future.

Although I'm guessing, it seems fairly certain that Palmby also was motivated to write his memoirs in an effort to influence the writing of the 1985 farm bill with his personal input. He strongly disapproves of the theme song, "Farm prices are made in Washington." Although his own predilections cannot be examined here, he does "recommend an end to CCC [Commodity Credit Corporation] loan rates on corn, sorghum and other feed grains" (p. 213). There are substantial reasons why we might disagree with his policy proposals, but we are indebted to Palmby for several case studies concerning important agricultural policies over the last 30 years.

The last two studies are certainly the most important in terms of the depth and sophistication of their analyses. They can be reviewed simultaneously because both volumes really comprise the proceedings of conferences at which, by and large, the same set of agricultural economists examined, analyzed, and elucidated the condition of the American agricultural economy and what should be done to improve it. Both conferences were designed to provide professional expertise, in the somewhat naive anticipation that this would be heeded by Congress as it proceeded to construct what came to be called the *Food Security Act of 1985*. As Ronald Knutson observes in *Alternative Agricuture*, "Farm bills are written by politicians—not by economists." However, he then proceeds to make the important claim that "the economists' role is to identify policy options and evaluate their consequences" (p. 418).

It is not feasible to try to review all of these papers, which have now become chapters followed by discussions. In *Alternative Agriculture*, political scientists would profit from a reading of D. Gale Johnson's paper, "The Performance of Past Policies: A Critique" (chap. 2). Alex McCalla's review of the preceding analyses of farm commodity policies (pp. 302–11) and Stanley Johnson's discussion of the alternative policy proposals (pp. 403–18) will lead the interested reader back to some of those papers for a more thorough examination. For some useful insights into the politics of American agriculture, political scientists should see Knutson's brief foray into political realism (pp. 418–22).

The centerpiece of *U.S. Agricultural Policy* is a paper by several agricultural economists at Iowa State University and the University of Missouri who jointly constitute a Food and Agricultural Policy Research Institute. Their influential and readable paper (pp. 119–94) is definitely recommended; indeed, all of part 2 ("Policy Options for 1985") could be designated for inclusion on the "required" list, especially John Schnittker's "synthesis" (pp. 201–4). Schnittker was a prominent United States Department of Agriculture (USDA) official in the Kennedy-Johnson administrations, and his policy insights are consistently impressive, a delightful intertwining of economic analysis and political realism.

Let us conclude with comments. The first concerns the rise in influence and prestige of agricultural economists over, say, the last 50 years or so. Bruce Gardner presents their case succinctly: "I believe that the opportunity exists to shape policy through analysis; for Congress, however motivated, wants to know the consequences of its actions (although it may not want them all published)" (Gardner, p. 15). To claim that agricultural economists "shape" policy seems to be something of an exaggeration; perhaps "expound on" would be a more accurate choice of verb. In any case, one could certainly argue that the members of the American Agricultural Economics Association have been extensively involved, over the last half-century, in efforts to influence policy makers to give credence to their analyses and legal sanctions to their prescriptions.

The second comment tends to modify the first. The research and scholarship of political scientists such as Theodore Lowi, Randall Ripley, and Grace Franklin (among numerous others) proved substantially more useful in predicting what actually came to be the principal provisions of the Food Security Act of 1985 than the valuable, but largely neglected, advice and counsel offered in the two conference proceedings reviewed here. The new farm law is another classic example of distributive politics; the iron triangle concept has not lost all of its analytical power, by any means. If political scientists and economists desire to become really influential in the rooms and corridors wherein policymakers negotiate and decide, we need to unite our disciplines in a much more effective form and manner. This fusion is underway, admittedly, but there is still very much to be done.

Ross B. Talbot

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Judges, Bureaucrats, and the Question of Independence. By Donna Price Cofer. (Westport, CT: Greenwood Press, 1985. Pp. xvii + 245. $35.00.)

This book about the role of administrative law judges (ALJs) in the Social Security Disability program moves along uneasily on two levels. First, it describes the battles in the continuing war among the administrators, politicians, and judges concerned with administering the program. Second, it criticizes the administrative process, primarily on the ground that ALJ independence has been threatened. The author sums up her point of view with the argument that

> If ALJs are not afforded adequate protection against bureaucratic, and therefore political, intrusions into their role, their objectivity will be —or will at least appear to be—compromised. (p. 35)

The book succeeds well in its first goal of describing the conflicts that have marked the last decade or so of the Social Security program. The first three chapters give us a general history of the program, of the decision-making process in government benefit programs, and of the role of ALJs. This provides the background for chapters 4 and 5, which thoroughly explain the conflicts from 1960 to the present. We learn about the Social Security Administration's growing concern with how the program was being administered, specifically with the ALJs' productivity, inconsistency, and high pro-claimant decision rates. We are told how the agency brought pressure on ALJs by withholding travel and staff perquisites, and how ALJs fought back in court to maintain their independence.

The author does not conceal the complexity of these battles. The agency's concern with productivity produced the mechanically applied "grid" for determining disability, which reduced inconsistency and improved productivity, but increased pro-claimant decisions. The agency's equating of pro-claimant decisions with decision-making errors is also criticized. Chapter 6 provides a detailed report on the results of a questionnaire sent to ALJs to obtain their views about these problems, and chapter 7 concludes with proposals for reform, including improvement of the state agency process, experimenting with more adverse ALJ proceedings, and adoption of a Social Security court.

The book is less successful in its second objective of criticizing the decision-making process. The rhetoric of the critique is that ALJ independence, and therefore claimants' rights, are threatened by a bureaucratic, and therefore heartless and political, administration. The characters in this morality play are too one-dimensional. Indeed, the strength of the book is its detailed description of the conflict among the actors, which is more complicated than the ALJ-vs.-bureaucracy rhetoric would suggest. It is not that the author's perspective is without support. The decision-making process at the state level is too routinized and the pressure from the agency on ALJs is excessive. However, the significance of the threat to the ALJs is not fully captured by viewing it as a simple dispute between the defenders of individual rights and intruding political bureaucrats. Put differently, the issues are less those of a morality play pitting individual against government, and more those of high political drama pitting those with different political views against each other, compounded by the fact that the ALJs' sense of professional self-worth is implicated by the definition of their role.

A somewhat richer view of these conflicts would point out that the Social Security Disability program is poised not-so-delicately at the conjunction of law, politics, and administration. It is not just that the definition of disability is unclear. The idea is itself the product of deep, even irreconcilable tensions arising from the public's conception of malingering, and from the uncertain distinction among the risks covered by unemployment, old age, and disability insurance. The disability program builds these tensions into its administration by placing independent judges at the end of the review process, as is true of the Tax Court in the federal income tax system. When this more complex perspective is adopted, the agency appears less heartless and politically intrusive, and more like any other agency trying to respond to shifts in political climate, often under congressional pressure. The clash between the agency and the ALJs becomes a dispute about interpretation, similar to what we might encounter if the Tax Court were inside the Internal Revenue Service rather than an independent court. The ALJs' problems are the result of housing them inside an agency, where political conflicts are inevitable.

WILLIAM D. POPKIN

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The State Politics of Judicial and Congressional Reform: Legitimizing Criminal Justice Policies. By Thomas Carlyle Dalton. (Westport, CT: Greenwood Press, 1985. Pp. xxi + 234. $35.00.)

The goal of this study is to understand what factors affect the degree of success with which criminal justice policies formulated at the federal level are implemented by states and, based upon these findings, to speculate more generally about political legitimacy and accountability in the United States. Toward this end, Dalton compares responses in four states (California, Colorado, Massachusetts, and Washington) to Supreme Court decisions concerning the rights of the accused and to executive and congressional policies concerning the quality of and access to criminal records.

The book's potential strength is its description of what transpires when the federal government is dependent upon the state for implementing policy. Indeed, the most satisfying parts of the book discuss how state courts, legislatures, and administrative officers grapple with federal policy—either to enforce it, subvert it, or simply understand it—and the motives of the various actors. Although one suspects that he makes too much of perceived distinctions in the political traditions of the four states to explain differences in behavior, Dalton does contribute to our understanding of the complexity that characterizes state implementation of federal policy. Furthermore, the discussion of how state and federal courts respond to one another and to other political institutions in their respective environments is thought provoking. We are provided with accounts indicating that the constraints faced by the state courts in executing federal policy vary, depending upon how the courts are perceived by others in their respective environments.

Unfortunately, the book does not live up to its fullest potential, for several reasons: careless usage; lack of clarity in the presentation and incorrect information; and unsubstantiated and overly broad claims. These flaws seriously undercut the author's credibility.

Interest groups and *elites* are referred to throughout the text, but the author makes little effort to define those attributes which might help the reader to identify an individual as either an elite or a member of an interest group. We are told that governors, attorneys general, judges, local elected officials, and state and local administrators are among the elite, but we are not told what characteristics distinguish them from nonelites. The difficulty created by this omission becomes important as we are introduced to a plethora of undefined elites, including political elites, national and state elites, policy subsystems elites, institutional elites, and administrative and professional elites. Sometimes individuals identified as elites function as interest group members; other times they are the target of interest group activity. We are told even less about Dalton's conception of interest group theory. This is problematic when, for example, he seems to equate the existence of an interest group (the ACLU in Colorado) with proof of its strength (p. 162).

In many ways, the most frustrating aspects of the book are the rather sweeping generalizations and cryptic remarks that are not warranted by the evidence presented. The first and longest chapter reviews the shortcomings of several major theoretical perspectives—occasionally with less-than-generous treatment of their proponents—and leaves the reader with high expectations about the utility of Dalton's version of "legitimacy theory [which] is uniquely suited to explain the outcomes of judicial and Congressional reforms in the administration of criminal justice" (p. 5). Yet in chapter 1, Dalton never offers more than what "appear to be several elements or conditions with which we could forge a separate theory of legitimation" (p. 36). Rather, we are asked to wait until the final chapter for a "reformulation" or description of legitimacy theory. This stylistic characteristic of making assertions but asking the reader to wait for a more thorough discussion and justification at a later point in the text is pervasive. Unfortunately, our patience is not very well rewarded. The closest we get to an "explanatory framework" is a variant of the systems approach, which owes much to David Easton, among others. Little if anything about the book is novel from a theoretical perspective, and Dalton seems to make too much of what he does offer. For example, he makes the unexceptional claim that, "one of the significant conclusions emerging from Chapter 6 is that federal policy originators can influence but are unable to control the state implementation of national policies regulating criminal justice processes" (p. 177).

Sentences or phrases are offered without appropriate context or are not justified by the material presented. For example, we are told the Massachusetts Supreme Judicial Court must continue "to conceal its underlying but long suppressed ideological principles" (p. 135), whatever they may be, and that "ambiguity can serve some important practical purposes, shielding the court from unpleasant political realities" (p. 140). The Colorado press, as an interest group, "held criminal records legislation hostage" (p. 141). Finally, much of the evidence presented to justify the interpreted impact of *Miranda* and *Mapp* on the states tends to be anecdotal interview data from anonymous officials.

While one might wish to argue that none of these errors in themselves damage the entire enterprise, together they create a penumbra of doubt about the care that went into this effort.

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Political Communication in America. By Robert E. Denton, Jr. and Gary C. Woodward. (New York: Praeger Publishers, 1985. Pp. xvii + 364. $15.95, paper.)

When Information Counts: Grading the Media. Edited by Bernard Rubin. (Lexington, MA: D.C. Heath and Co., 1985. Pp. xiii + 244. $23.50.)

These two volumes join a growing list of works that take seriously the mutual exchange between communication and politics in contemporary society. Although differing considerably in origins and purposes, they complement each other in overall content and emphases.

As professors of communication at Northern Illinois University and Trenton State College, Denton and Woodward represent an expanding population of communication scholars addressing the cross-disciplinary concerns that bring together students of politics no matter what their academic training or appointment—i.e., those in political science, speech communication, journalism, sociology, and cognate disciplines. Noting that "the communication analyst is always a guest (if not an intruder) on someone else's turf" (p. vii), the authors have written a textbook summarizing the roles and functions of communication in U.S. politics.

Because they believe communication to be prior to all other fields of inquiry, it is not surprising that the authors adopt what some observers may well regard as an imperious definition of their subject matter. Political communication, they write, is *"public discussion about the allocation of public resources (money), official authority (who decides), and official sanctions (what is to be rewarded or punished)"* (pp. 15-16). It appears, however, that they quickly draw back from asserting that communication is all of politics—i.e., that communication *is* the authoritative allocation of values—by asserting that political communication is a "valuable mediating agent" (p. 18) that makes it possible to draw up short of violence, promoting instead compromise, public acceptance, and orderly change.

Augmenting that definition with a framework borrowed from symbolic interactionists, the authors proceed to trace the role of communication in a variety of political settings—electoral campaigns, the presidency, bureaucracy, Congress, and mass media. There are, of course, threats to open political discourse—coercion, deception, mystification, etc.—and so the authors close with a warning that unfettered political talk, despite its inefficiency, is essential to free government. That this line of argument frequently has been repeated before, the authors freely admit. So freely, in fact, that one of the overriding characteristics of the book for those who might employ it as a course text is that it offers a compendium of virtually everyone, practitioners as well as scholars, who have ever written on the subject. Whether undergraduates reading the text will be as drawn to such graybeards is another matter.

Denton and Woodward end their text decrying the corruption of political communication. Rubin's edited volume is devoted primarily to that topic alone. The 13 contributions comprising this work examine the role communication plays in shaping the power and impact of all manner of political actors—democrats, demagogues, and dictators. The essays grew out of a series of panel discussions organized by the Associates in Research for Public Reporting. As a result, the individual selections mesh around common themes in a fashion superior to that characteristic of most edited volumes.

Rubin, professor of governmental affairs and communication at Boston University, has organized selections around three themes. The first directs attention to why we might have legitimate concerns regarding whether political information actually serves democratic ends. The second theme emphasizes areas of endangered free speech, and the third focuses primarily upon the news media role in covering, explaining, and perhaps exacerbating problems. The topics of some articles repeat points made many times over—for example, the use of buzzwords in news coverage, George Orwell's prophecies, the role of cameras in the courtroom, and how television covers international crises. More rewarding are Rubin's critical updating of Walter Lippmann's notion of stereotypes in the electronic age, Ralph Rosnow and Marianthi Georgoudi's systematic analysis of the part played in politics by small talk and idle gossip, and Walter Clemens's critique of the intellectual foundations of the Reagan policy toward the Soviets.

Two of the audiences for this volume, according to Rubin, are identical with those addressed by Denton and Woodward: first, college and university students, and second, their professors. Given the vertical, issue-directed and case-centered orientation of the selections, one suspects that Rubin's edited work will appeal far more to professorial inclinations, whereas the horizontal, highlighting style of Denton and Woodward is better suited to the yellow felt pens of likely undergraduate readers. Taken together, the two works reintroduce what are fast becoming the staples of political communication. It remains to be seen whether more is to be said or, as with so many cross-disciplinary fields in the social sciences, this one has already peaked.

DAN NIMMO

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Government and Sport: The Public Policy Issues. Edited by Arthur T. Johnson and James H. Frey. (Totowa, NJ: Rowman & Allanheld, 1985. Pp. x + 275. $38.50.)

In the past it was common to find the sports pages of newspapers filled with stories about which teams won and lost and which players performed well, and with predictions for upcoming games or seasons. These stories would serve as the focus of debate for sports fans. Now, however, a glance at the sports news and eavesdropping on conversations among fans will produce a different mix of topics: discussions of strikes, antitrust, television contracts, law suits, Title IX, boycotts, graduation rates, franchise movements, and a host of other policy issues. In this changed environment it is hardly surprising that considerable scholarly interest is now being given to issues in professional and amateur sports. The volume *Government and Sports* by sociologist James H. Frey and political scientist Arthur T. Johnson includes research from a variety of disciplines on a range of issues confronting sports today. Holding this diverse collection together is the linking of each issue to the role that government now plays in the regulation of professional and amateur sports, and the alternatives to government involvement in each of the issues under consideration.

Johnson and Frey organize the volume's 13 articles into three sections. The first is comprised of those that address policy issues and athletes. It includes pieces on labor negotiations between owners and players, the status of the amateur athlete as an employee, the impact of Title IX on women's sports, regulation of sports agents, and policies dealing with sports violence. By contrast, the second section addresses issues of sports administration such as the legal status of NCAA rulings, the application of antitrust laws in professional sports, sports broadcasting policy, and the effect of tax policy on sports. The final section focuses on what the editors label "public interest" and sports—actually a catchall section for three unrelated issues of sports and public policy: gambling, franchise relocation, and foreign policy in sports.

In their introduction, Johnson and Frey claim that the purpose of the book is to illustrate how public policy affects sports in the United States. Accordingly, each essay is designed to answer three questions: (1) "What is the proper role of government relative to sports?," (2) "Is this specific sports issue an appropriate issue for government to resolve?," and (3) "Is government action capable of bringing about a resolution of the issue before them?" (p. 14). To a significant extent, all of the essays attempt to meet these goals, but the real value of this collection is in the substantive information one can gain about the policy issues involved. The collection is as up-to-date as any such effort could be expected to be. Each issue is carefully defined and presented in a fashion that does not assume the reader possesses great prior expertise. Yet the authors fully explore each issue's complexities. Of particular value are the extensive citations at the end of each chapter, because much of the material cited would not necessarily come to the attention of political scientists. In sum, these essays correspond to good law review articles.

It is unlikely that readers will find all the essays of great interest unless they are interested in sports policies in general. It is far more likely that those engaged in policy research will find the discussion of particular issues in relation to sports—tax policy, antitrust policy, civil rights, etc.—to be of specific use to their research. Moreover, many of the contributors are not social scientists. What links there are to broader policy theory come primarily from the editors' introduction and conclusion. Frey and Johnson do try to evaluate the potential for government regulation for each of the sports issues examined in the book in terms of James Q. Wilson's ideas on the concentration of costs and benefits. Do not, however, expect to find significant attention given to theoretical underpinnings.

There are three articles that will be of particular interest to faculty members concerned with the conduct of intercollegiate athletics. Allen Sack and Bruce Kidd analyze the arguments and court cases dealing with the employment status of scholarship athletes. They contend that scholarship athletes are employees and should be entitled to the rights that employees enjoy. In a study of the impact of Title IX, Linda Jean Carpenter finds that its implementation has produced several unintended consequences. Brian Porto offers an enlightening analysis of the shortcomings of the NCAA and the alternatives available to other participants in the development of intercollegiate athletics policy to address the situation.

Government and Sport offers something of value to a broad range of readers. In this sense, the book's diversity is one of its major strengths. This diversity is also weakness, however, because it is unlikely that many readers will be interested in more than a few of the substantive issues covered in these essays.

BRUCE I. OPPENHEIMER

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Protest: Studies of Collective Behavior and Social Movements. By John Lofland. (New Brunswick, NJ: Transaction Books, 1985. Pp. xii + 349. $24.95.)

This book is a collection of papers and previously published articles by a sociologist well known for his perspicacious and original analyses of the Unification Church (UC) in its infancy. These and his more theoretical articles on contemporary religious movements remain the best chapters in what is essentially a potpourri of pieces, some of which have nothing to do with protest, and not much more to do with collective behavior and social movements.

It is unfortunate that Lofland did not use his prior writings as a framework from which to produce an integrated book. Nonetheless, his analyses of the UC and other cult organizations, though not as transferable to political movements as he would have his readers believe, do provide much food for thought. The most insightful section for political scientists is that on conversion, which contains two studies of the UC and one theoretical chapter. Conversion is a process rarely analyzed by political scientists other than in its most extreme form—brainwashing. Yet some form of conversion is necessary for every political organization seeking to recruit supporters to nontraditional points of view. Lofland provides both a definition and a complex model of conversion—illustrating six different types—along with case studies that illuminate some of them.

His section on organization tries to identify types of social movement organizations, but his data are almost entirely based on religious movement organizations. Since religious groups fulfill different functions than political groups, and are rarely concerned with capturing state power or changing public policy, the applicability of his analyses to the political realm is limited. Nonetheless, this chapter is helpful in understanding why some political groups, usually extremist ones, do resemble religious movement organizations.

The section on protest actions is the weakest. Its major emphasis is on interest group activities such as lobbying and networking. While one could argue that these are forms of protest, it does stretch the word beyond its usual meaning. Since the types of actions to which the term *protest* is usually applied are given only cursory attention, Lofland's analyses of lobbying appears idiosyncratic rather than part of a larger framework.

JO FREEMAN

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Social Research in the Judicial Process: Cases, Readings and Text. Edited by Wallace D. Loh. (New York: Russell Sage Foundation, 1985. Pp. xxix + 736. $37.50.)

Social Science in Law: Cases and Material. Edited by John Monahan and Laurens Walker. (Mineola, NY: The Foundation Press, 1985. Pp. lix + 521. $32.50.)

These texts contain a wealth of empirical research and judicial decisions illustrating the relationship—or the lack thereof—between social science studies and legal policies. Fortunately, the books approach this relationship from different perspectives, and thus overlap only minimally. Loh's book is probably of more interest to political scientists, because it is primarily aimed at the social science student and because it reprints considerably more research than case law. Readers are assumed to know little law, and are given an elementary introduction to the legal process and the case method. Monahan and Walker's text is explicitly written for law students and professionals, and begins with a primer on social science research methods. Nonetheless, much of their material can be useful for social scientists.

Loh focuses largely on the major public law controversies. The famous dispute over the use of sociological studies in *Brown v. Board of Education* (1954) is covered, as is the ongoing debate over the degree to which social science has informed particular busing decisions or illuminated what the racial integration of schools has accomplished. In the criminal justice area, studies of the impact of *Miranda v. Arizona* (1966) are analyzed, as are those of the exclusionary rule in search and seizure. Research into the efficacy of capital punishment is also included.

Over a quarter of Loh's text focuses on jury research—e.g., selection procedures, how decision rules affect the outcome, the effect of exposure to pretrial publicity, and jurors' reactions to the nature and order of the presentation of evidence or the demeanor of witnesses—and this may be of less interest to political scientists. Juries are a frequent object of social science research, especially among social psychologists, probably because jury situations can be simulated fairly easily. However, the results of jury studies make a more valuable theoretical contribution to research on small-group behavior, and a more practical contribution to lawyers operating regularly in the criminal justice system, than they do to either a theoretical or practical understanding of the formation or implementation of substantive legal policies.

Loh uses three conceptual approaches to the research and policy relationship. The first looks at research that occurs prior to an important judicial decision and informs (or could inform) the judges' minds. The "Brandeis Brief" and Kenneth Clark's famous black and white doll experiment, cited in *Brown v. Board of Education*, are examples. The questions crucial to this approach are when, why, and to what extent such research influences the making of judicial policy. Generalized answers are difficult, of course, but Loh offers few suggestions or insights. The second approach focuses on research that presumably interacts with changing judicial policy over time. The Coleman report and research into public attitudes toward capital punishment are examples. This is probably the approach most in need of exploration, because such interaction is necessary if social science research can be said to have anything more than a sporadic influence on judicial policy making. Unfortunately it is still difficult to draw much in the way of conclusions about the whys and wherefores of such influence. Impact studies—how court decisions affect other governmental agencies—constitute the third approach. Loh limits his focus to the police, and does an excellent job of reviewing the literature and offering insights and tentative conclusions.

Loh's in-depth examination of a few major public law controversies largely embodied in Supreme Court decisions is complemented by Monahan and Walker's eclectic format, which offers something for everyone. Their material goes well beyond public law, exploring such areas as the impact of malpractice decisions, trends in child custody cases, and how research is used in trademark violation suits. Sometimes, in fact, the research they reprint or cite relates to practical problems rather than court decisions. Monahan and Walker discuss the validity and utility of hijacker and rapist profiles, methods of deterring shoplifting, and the susceptibility of youth to pornography, among other things. Three major focuses organize their text: research on legal issues, research concentrating on the behavior of actors, and research about institutions and interactions in the legal process.

Both texts should prove to be valuable resources to those interested in the judicial process or in the more general area of law and society. Their simultaneous appearance is as welcome as it is fortuitous. For a long time, no one has seen fit to put together a rich compendium of law and social science material. Now we have two.

BRADLEY C. CANON

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New Communication Technologies in Politics. Edited by Robert G. Meadow. (Washington, D.C.: Annenberg School of Communications, 1985. Pp. 145. $15.00, paper.)

In early 1983, the Natural Gas Supply Association (NGSA) contracted with Matt Reese, a prominent consultant in targeted communications, to generate a grass roots campaign on behalf of decontrol of natural gas prices. Everyone realized that the task would be difficult; most of the congressmen on the committees that would take up this legislation represented constituencies that had little to gain from decontrol. Reese's strategy was simple. Identify and contact those citizens in each of the 15 targeted districts who should be most sympathetic to a decontrol appeal, and then recruit them to drum up a letter-writing campaign among their neighbors. The budget for this first foray in what NGSA foresaw to be an extended public relations offensive was $1,000,000—about $70,000 for each targeted district. Revealing an irony of modern politics, a representative of the gas lobby confessed to a reporter, "None of these grass roots efforts comes cheaply."

Several types of respondents were identified in surveys to be susceptible to a gas producers' appeal. One whom Reese labelled the *Mid-America Blues* group was distinctive in its hostility to "politicians in Washington." Apparently, they were also held to be sufficiently distinctive demographically to justify targeting with census data. The only apparent problem in recruiting them behind the NGSA cause was their equal hostility to the oil and gas industry. Reese's recommendation: in appealing to these citizens, "Don't even hint at a connection to the gas companies if you can avoid it." Hence, the Alliance for Energy Security was born. Behind this alias, the NGSA fashioned the following message, which it sent to thousands of citizens: "Why do we go to Algeria for something we've got in [target's state]? . . . Because those guys in Washington thought they could fix prices on natural gas." These mailings were followed up with phone calls soliciting their participation in a letter-writing campaign.

By early August, over 38,000 kits of flyers and "constituent" postcards were in the hands of volunteers who were instructed to distribute ten letters each on a designated Saturday. By most indicators, the campaign failed to meet the sponsors' expectations. On the morning the volunteers were to hit the pavement, the *Washington Post* ran a front-page story unmasking NGSA. One congressman subsequently reported that some of those who had sent in the inspired postcards later wrote to say they had been duped. Another targeted representative recorded only 1,125 NGSA postcards, which thus cost about $60 each.

Whether one finds in this story cause for alarm or reassurance in either the vigilance of the press or the inertia of the American public, it illustrates the extent to which political actors in Washington are employing modern technology to mobilize public support. *New Communication Technologies in Politics* introduces us to some of the individuals and techniques that are changing the way campaigns are waged in this country. An outgrowth of an Annenberg conference of leading communications consultants, the book is, unfortunately, ill suited to do much more than make introductions. There is too little assessment of effects on voters or candidates and no consideration of implications for democratic politics. With practitioners writing the essays, one does not find, understandably, troublesome stories such as that of the NGSA campaign. Instead, the consultants offer inflated claims for the power of this new technology and the good it achieves. Reese, for example, cites the NGSA campaign in asserting that his targeting "approach which had worked so well with candidates for office and referenda issues yielded almost incredible results when applied to get grass roots response to lobby members of Congress" (pp. 106-7). Hmm.

The only serious attempt to validate the proprietary methodologies presented in these essays is offered by Kevin L. Kramer and Edward J. Schneider in describing Custom Targets, the trade name of Market Opinion Research's (MOR) demographic targeting system. Using standard benchmark surveys rather than Claritas's preformed consumer groupings as the basis for targeting, MOR's concept appears clearly superior to that used by Reese and others. Still, Kramer and Schneider are not fully persuasive that their approach works as well as they say, or is cost effective. They show that targeted precincts did deliver a larger vote for their side, but is it because targeted voters were more sympathetic in the first place? After all, receptivity to the message is what qualified them as targets. One wonders, as must campaign managers, whether the money spent for a high-priced targeting consultant could have been put to better use in sending out more messages, even if it meant doing so less efficiently.

Despite its inherent limitations, *New Communication Technologies in Politics* offers a useful introduction to this new field of political consulting. As targeting and communications technology continue to improve, the wares offered by these consultants will become increasingly attractive to candidates and others who wish to mobilize public opinion. As campaigns become better endowed, more candidates will find these services affordable. Most importantly, as the brand loyalty of voters continues to weaken, candidates will become ever more dependent on methods developed by advertising and marketing research to identify their message and to locate their supporters. For these reasons, even a book with such modest aspirations as this deserves our attention.

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Religion in American Public Life. By A. James Reichley. (Washington, D.C.: The Brookings Institution, 1985. Pp. x + 402. $31.95, cloth; $11.95, paper.)

The First Liberty: Religion and the American Republic. By William Lee Miller. (New York: Alfred A. Knopf, 1986. Pp. 373. $25.00.)

"The First Amendment is no more neutral on the general value of religion than it is on the general value of the free exchange of ideas or an independent press" (p. 166). The purpose of the amendment, James Reichley goes on to assert, was partly to reserve to states the more delicate questions related to the practice and even the promotion of religion. For this reason, Reichley finds the Supreme Court's establishment cases ill conceived. Although he accepts the first two recent tests of establishment—i.e., purpose and primary effect—he thinks the Court's willingness to address these issues "was neither constitutionally necessary nor consistent with American tradition" (p. 156). Reichley attributes part of the Court's problem to a judicial "non-interpretivism" that forgets the words of the Constitution.

Religion in American Public Life begins with a review of religion from the first settlement to the founding. Next is a critical account of relevant Supreme Court decisions, followed by an extensive, effectively detailed, and richly documented analysis of the political role of religion up to the present. A final chapter discusses appropriate political roles for churches.

Reichley, a senior fellow at the Brookings Institution, combines an explicit respect for theistic-humanistic religion with concern for democracy. In America, better-educated people commonly see formal religion as a threat to democratic values. Religion is considered untrue, provincial, and either too conservative, as in the abortion controversy, or too liberal, as in utopian solutions to nuclear problems. Against this prevalent negative opinion about religion in public life, Reichley argues that the same religions that can be dangerous are, nevertheless, essential for the very survival of democracy.

The problem for Reichley is the difficulty of reconciling cultivation of respect for persons with dedication to politics. Reichley challenges the assumption that the presence of persons with reasonable good will will suffice. There are too few such people, and if they are put in charge they will create, perhaps, a benevolent aristocracy of some sort, but never democracy. Respect for common people and their government requires, says Reichley, an independent source of authority and influence that helps people to do right and restrains their leaders. The public role of religion in America has been, at its best, to provide this foundation for liberty and order. Reichley's contribution is to discuss with great intelligence and sensitivity the role of religion within a radical and even pessimistic understanding of American democracy.

The First Liberty is a bicentennial celebration of the constitutional provisions for religious liberty. When the American people agreed that the supreme law of the land should not favor a particular religion or require a religious test for public office, they set religion free to play an expanding, and generally positive, role in American life. Miller concludes his review of the founding, colonial antecedents, and recent developments in politics and law, with a prediction that we can expect Jewish and Roman Catholic communities, especially, to make significant new contributions to our politics.

Miller has written a distinctive book designed to capture the attention of tired or otherwise inattentive readers. He uses a personal style without endnotes, and only an occasional bibliographical reference. This deliberate informality recaptures the intense feelings involved in an appropriate appreciation for the historic achievement of religious liberty. Focusing on dramatic episodes (e.g., disestablishment in Virginia), unusual writings (e.g., Roger Williams's *The Bloody Tenent, of Persecution, for Cause of Conscience*), and great leadership (e.g., James Madison's work), Miller rescues this story from historical oblivion.

Miller gives dissenting Protestantism credit for establishing religious liberty and promoting voluntary public service, but then blames this tradition for much of the individualism and political indifference in contemporary America. The Supreme Court, in its recent decisions on establishment, receives a less caustic judgment. Miller weighs the complexity of current decisions involving church and state and their attendant debates. Remaining reasonably neutral, he counsels calm reflection from diverse perspectives. History and past law provide for him few normative principles for unravelling the complex ties of church and state.

Trained in religious social ethics, Miller has long been active in interfaith and interdisciplinary discussions of religion in America. A former political science professor, he is now Chairman of the Department of Rhetoric and Communications at the University of Virginia.

These volumes on American politics and religion join those by Booth Fowler, Charles Dunn, and others soon to be published, adding depth to our understanding of this no-longer-ignored, much-to-be-studied subject. Miller, who is more liberal, and Reichley, who is more radical, have given us two excellent books.

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Factions in House Committees. By Glenn R. Parker and Suzanne L. Parker. (Knoxville: The University of Tennessee Press, 1985. Pp. xx + 312. $26.95.)

Committees remain at the very center of congressional deliberation and decision making. Until 1971, knowledge of committee processes largely reflected the reports of astute "inside observers" (e.g., Fenno and Price); in that year, however, as the Legislative Reorganization Act of 1970 mandated, committees began to record and make available their roll-call votes. Glenn and Suzanne Parker, almost alone among students of Congress, have seized this opportunity to chart the character and continuity of factions in House committees. Using factor-analytic and correlational techniques to analyze the substantive (amendment) votes in 19 panels, they compare the factional alignments in the 93rd and 94th Congresses (1973–1976) with those in the 95th and 96th (1977–1980). They seek to describe the lines of committee division in terms of party, ideology, executive influence, and constituency pressure, and to account for changes in these divisions over time. In doing so, they have produced a most valuable study.

The bulk of the authors' analysis extends their pioneering paper ("Factions in Committees: The U.S. House of Representatives," *APSR* 73 [1979]:85–102). They find partisanship and ideology to be the most significant correlates of committee voting alignments. On some panels (e.g., Budget), the two factors coincide, pitting highly polarized factions of conservative Republicans and liberal Democrats against one another. On other bodies (e.g., Judiciary) ideology diverges from partisanship, producing more diffuse blocs of conservatives and liberals. Executive branch forces influence committee cleavages over and above partisanship and ideology on five committees (e.g., Appropriations). When the executive is concerned and employs its persuasive resources, committee factionalism reflects those efforts; because these efforts "crosscut" other forces, the factions on these committees are seldom tightly polarized.

Constituency factors also are significant supplements to party and ideology on three committees. Where interest groups "are interested in many issues falling within the committee's jurisdiction," they "tend to be sustained and pervasive forces within the committee's environment" (p. 158), and factions reflect support for and opposition to group policy preferences. The Agriculture and Banking committees are cases in point. For the most part, factional patterns endure across the 1970s. Committee jurisdictions change little, committee agendas are relatively fixed, and the types of members who serve remain constant, as do their voting dispositions; in consequence, voting alignments are stable. The few changes that do occur flow from the appearance of extensive membership turnover, and a new administration in the White House. On balance, however, voting factions in the

Nixon-Ford and Carter presidencies look very much alike.

In a truly ingenious analysis (chap. 7), the Parkers explore the relationship between committee cleavage and coalition formation. They hypothesize that "the more partisan loyalties of committee members are crosscut by other environmental influences (specifically ideology, administration, and constituent-group interests), the greater will be the proportion of larger-than-minimum" winning coalitions on the committee (p. 247). "Polarized-partisan" panels—those on which environmental influences reinforce partisanship—do indeed tend to form minimum-winning coalitions of between 50% and 60% of the members. "Nonpartisan" committees, by contrast, tend to develop "universal" coalitions (of more than 70% of the members), and "mixed-partisan" committees, on which there exist significant crosscutting environmental forces, display winning coalitions of intermediate size. During the Carter years, coalitions tended to be smaller, suggesting that divided government requires the president to reach across party lines to secure opposition support, thus producing more universalistic voting alignments.

It is possible to quibble about some methodological choices the authors make. Their decision to examine only members who served on a committee continuously during each pair of congressional terms serves to reduce the pool of legislators in their analysis, as does their decision to eliminate those who voted on fewer than 70% of the substantive roll calls in committee. Thus, to cite an extreme case, their analysis of the Agriculture Committee in the 93rd and 94th Congresses contains only 18 of the nearly 60 members who served on the panel between 1973 and 1976. As a result, they may overestimate the stability of factions, because they consider only those representatives—the high-participant, continuing members—most likely to behave consistently over time. Conversely, the authors, because they exclude the relatively inactive, noncontinuing members who may behave more erratically, may underestimate the diversity of committee factions.

The Parkers provide graphs—"two-dimensional representations"—that *"generally"* locate members of the committee with respect to "the major cleavages within Congress and its committee system—party and ideology" (p. 32, emphasis added). This implies that in some instances other factors—presumably executive, constituency, or policy influences—constitute the fundamental dimensions of committee cleavage. The failure to label the axes of these graphs prevents identification of these latter circumstances. Similarly, the authors make much of polarization of committee factions, and while visual inspection of the spatial location of members gives some clues about the degree of polarization, a statistical measure of dispersion might have made their contrasts between "tight" and "dispersed" clusters more readily interpretable.

A quantitative study of roll-call votes nicely illustrates the difficulties of specifying cause and effect. The authors, who repeatedly acknowledge the problem, often resort to inference and speculation to disentangle the impact of executive and constituency forces from that of party and ideology. They rely on personal data on committee members, possession of leadership (including subcommittee) positions, and the existence of pertinent policy divisions in the panel as evidence that a particular bloc is sensitive to administration or constituency-group pressures. They also draw on previous nonquantitative work to identify committees susceptible to such influences. These sources permit plausible, but not always altogether persuasive, arguments for executive or constituency impact on particular committee factions. In the last analysis, it is never certain whether a cluster whose members have high presidential support scores is responding to executive leadership or voting out of partisan or ideological compatibility with a president of their own persuasion. There is a moral here, of course: neither quantitative nor more conventional methods alone are likely to provide definitive answers. Taken together, however, as complementary approaches, they can offer fuller, independent, and perhaps more convincing explanations of important phenomena.

It is exactly here that the exceptional value of the Parkers's work becomes crystal clear. They have brought a new, untapped data source to bear on a set of central questions about congressional committee politics. If they have often confirmed the conventional wisdom (from an independent data base), they have also challenged that wisdom in numerous instances. Their study has identified committee voting patterns precisely, advanced arguments to explain those patterns, and made a distinguished contribution toward understanding coalition formation. These are no small accomplishments. Moreover, they have, implicitly at least, set a research agenda for the study of committee politics, and if scholars now take up the challenge, as they surely will, it will be because *Factions in House Committees* has led the way.

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Organized Interests and American Democracy. By Kay Lehman Schlozman and John T. Tierney. (New York: Harper & Row, 1986. Pp. xix + 448. $12.00, paper.)

Scholarly interest in the nature and role of organized interests in American politics, awakened by a realization that pressure groups are alive and well and, indeed, flourishing, has recently enjoyed a renaissance. Perhaps the best indicator of the subject's panache is the revival of the market for textbooks on interest group politics. After a hiatus of a decade, publishers have brought forth at least four new interest group texts in the last four years. The latest of these is *Organized Interests and American Democracy*, by Kay Lehman Schlozman and John T. Tierney.

In conception, *Organized Interests* is as much a reportage of research results as a comprehensive text. The book marks the culmination of a five-year research program wherein the authors conducted long interviews with 175 organized interests—citizens' groups, professional associations, labor unions, trade associations, and corporations. The results are the centerpiece of the book, both in presentation and in substance. Schlozman and Tierney provide a detailed snapshot of who takes part in the pressure system of the 1980s, what the organizations amount to, and what the organizations do. Their data confirm many casual observations—that pressure activity has increased, for instance—but disconfirm other notions—for example, that public interest groups have become more prominent. Researchers and teachers will find their descriptive information valuable in itself.

The Washington Representatives Survey is also this textbook's strongest point, for Schlozman and Tierney, without making a big deal of it, have demonstrated one of the things political scientists do, namely, bring systematic evidence to bear on empirical and theoretical puzzles. As the authors note,

> The realm of organized interest politics is so vast . . . that it is possible to locate an example to illustrate virtually any reasonable generalization one might put forward. Only by taking a more global view can we get a sense of the relative frequencies within this world of astonishing political diversity. (p. xiii)

The central descriptive theme of the book is that today's Washington pressure activity is "more of the same." The authors find that the number of groups in the capital has burgeoned, and that groups that were already part of the Washington landscape have stepped up their activity. The proportion of organizations representing particular constituencies, however, has stayed remarkably constant, with the notable exceptions of unions, which have declined in prominence, and corporations, whose share has jumped markedly. Moreover, they assert, group tactics remain Congress-centered and no more or less reliant upon such traditional strategems as regular contact with public officials, testimony at hearings, and grassroots lobbying.

For the most part, the authors are appropriately modest about the uses to which their results can be put, although they have a tendency to take their respondents' answers at face value. (What Washington representative, for instance, would denigrate the value of a good reputation as a political resource?) To their credit, however, Schlozman and Tierney have sought to lessen their reliance on the recall of Washington lobbyists by collecting a variety of side evidence, ranging from anecdotes to a census of interest groups active in 1960, to a clever experiment starring Schlozman's infant son. Nor are the authors so insistent upon "more of the same" that they neglect nuances in the sameness. Although citizens' groups have not become more prominent in the universe, they find, the composition of the sector has changed from dominance of civic groups to primacy of environmental and consumer groups.

As one might expect, the empirical and topical sweep of *Organized Interests* has been purchased at a price. The authors have stayed fairly close to their data, and it does a serviceable job of covering the bases, including a few I had not expected (such as the internal democracy of groups), but it keeps them at arm's length from others. Lacking here, as elsewhere, is a serious treatment of power and influence. Granted, power is a large, digressive topic for a book on organized interests, but students must wrestle with what power and influence are if they are to avoid the promiscuous attributions of influence that so plague many popular and scholarly treatments of interest group politics.

The very scope of *Organized Interests*, moreover, contributes to its least satisfactory aspect. Schlozman and Tierney ably review and evaluate the major theories of group membership, internal dynamics, and organizational maneuverings in elections, Congress, the bureaucracy, and the courts, but they have chosen a theoretical theme that is inadequate for the task of integrating these disparate elements, and the book lacks a sense of how all the pieces fit together.

The authors pose an interpretive dichotomy. On one side is the pluralist model of pressure politics (associated with Truman) that sees government as the very expression of the push and pull of contending interests. On the other side is the revisionist model (associated with Bauer, Pool, and Dexter) that portrays organized interests as actors so put-upon and cross-pressured as to be ineffectual. The degree to which American government is the captive of particular interests *is* an important question; that, presumably, is why we care enough about organized interests to put so much into studying them. Yet the only defensible conclusion when the dichotomy is so posed is the one that Schlozman and Tierney arrive at—that the reality of pressure politics is far more complex than either the pluralist or revisionist model can accommodate. What we are left with, then, is complexity. In a sense, we are back to square one.

X

So what might have been better? It is quite unfair, after all, to expect anyone to develop the grand theory of organized interests in American politics. The answer, I think, is to be at once more limiting and more speculative. The task is to draw relational linkages—to treat interest groups as modes of citizen participation, interest groups as policy actors, and interest groups as problems in democratic theory as related questions, instead of as discrete questions. How, for instance, might the maintenance needs of political organizations structure their dealings with government elites? How might the goals of legislators, bureaucrats, and judges shape their responses to associational demands? What are the normative implications of different answers to these two questions? In short, once we know something about what an organized interest is or does, how does that help us understand something else it is or does?

Texts on interest group politics must be both empirically and theoretically informative, and this book is. They must also show how political scientists reason about how organized interests and policy makers are likely to behave, and about what difference it makes. Students should learn not only about the complexity, but about the if/then thinking that political scientists use to draw pattern from chaos.

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Administrative Discretion and Public Policy Implementation. Edited by Douglas H. Shuvamon and H. Kenneth Hibbeln. (New York: Praeger Publishers, 1986. Pp. x + 308. $40.95.)

The causes, uses, abuses, and strategies for controlling administrative discretion in public policy implementation have been frequent areas of inquiry and research in the literature on public administration, public policy, and administrative law. This well-written collection of 17 essays provides more grist for the mill. The editors, Shuvamon and Hibbeln, have compiled a collection of essays that address the gamut of issues raised by "administrators possessing discretionary authority" in policy implementation (p. 8).

Shuvamon and Hibbeln organize the 17 essays around six perspectives/sections. Each section contains at least one case-analytic essay and at least one essay of more general relevance. The editors' introduction and prefaces to each of the six sections provide a brief overview of each perspective. These could have been more expansive and provided more theoretical links. The six sections are on (1) the legal context of administrative discretion; (2) the causes of legislative delegation of authority; (3) administrative discretion in regulation and enforcement; (4) the politics/administration dichotomy; (5) the effects of the intergovernmental system on administrative discretion; and (6) traditional and other responses to administrative discretion.

The essays in the first section explore the legal framework of administrative discretion, and would be of most interest to students of administrative law. One of the few theory-rich essays of the volume is found in the section on delegation. In this essay, Angus MacIntyre delineates 18 sources of statutory ambiguity and systematically assesses the difficulty these ambiguities pose in attaining democratic accountability.

Both essays in the third section provide substantiation of the empirical invalidity of any dichotomized formulation of the relationship between politics and administration. William Browne provides data on the policy leadership of city managers in Michigan. T. Zane Reeves recounts the 1970s struggle between ACTION political appointees and career bureaucrats over shifting the mission of the agency from an antipoverty to a volunteerism focus. In one of the two essays of the fourth section, John Scholz applies tit-for-tat contingency theory to "explain how discretionary enforcement can increase cooperative compliance" (p. 145).

The essays of the final two sections add to the extant literature on administrative discretion in policy implementation, and offer insights into the adaptive behavior of administrators, administrative agencies, the judiciary, and legislators. The intergovernmental relations selections reveal the impact of the block grant in decreasing the discretionary authority of federal, state, and local bureaucrats. In the final section on responses to administrative discretion, five essays describe and critique a "comprehensive array of political and legal efforts to assure beneficence and limit the abuses that ensue from the exercise of administrative discretion" (p. 209). Included in this section are case studies on the Interstate Commerce Commission and the Economic Development Administration; an empirical analysis of the impact of the now unconstitutional legislative veto on administrative-legislative interaction; and more general discussions of such control techniques as the Administrative Procedure Act, Freedom of Information Act, and judicial review.

The richness and breadth of this collection are evident in its diversity of policy arenas (mental health, antipoverty, science, immigration, and economic development, to name a few); variety of types of administrator (federal, state, and local bureaucrats; city managers; street-level bureaucrats); and range of methodological approaches (case studies, descriptive typologies, bivariate statistical analyses, and game theory). In addition, the essays are well balanced. Most of the authors acknowledge that administrative discretion is problematic. However, several also attribute positive outcomes to such discretion. Noteworthy in this regard are the lucid discussions by Douglas F. Morgan, John A. Rohr, and George J. Gordon on how administrative discretion may increase government responsiveness and accountability.

This volume does not offer any tried-and-true strategies of how to structure or confine administrative discretion for better public policy implementation. It is, however, a wealth of very readable, case-specific information on one of the most important and enduring issues of public administration theory and practice.

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Constitutional Reform and Effective Government. By James L. Sundquist. (Washington, D.C.: The Brookings Institution, 1985. Pp. x + 262. $26.95, cloth; $9.95, paper.)

In December 1979, C. Douglas Dillon, former secretary of the treasury, under secretary of state, and ambassador to France, declared,

> I very much doubt that . . . we can long . . . afford . . . the division of power and responsibility between our Executive and Legislative branches of government. . . . I have no pat answers. But I do know that until we are prepared to examine the basic structure of our federal system . . . rather than indulging in continuous . . . recriminations, our problems will

> remain . . . and, in all probability, increase in severity. (Reston, James, December 23, 1979. Where Are We Going? *New York Times*, p. 3-13)

This speech led to the organization of the Committee on the Constitutional System (CCS) in 1981. Members combine reverence for the constitutional ideal with a conviction that the uniquely favorable circumstances of the provenance and evolution of our Constitution's particular structures have virtually disappeared.

Now CCS charter member James L. Sundquist publishes a magisterial account of constitutional origins and the history of suggested reforms, and an intensive and detailed analysis of contemporary proposals. With regard to the third topic, he deals (1) with forestalling divided government (the "team ticket" that mandates straight-ticket voting for president and Congress, bonus congressional seats to insure the winning president a majority, etc.); (2) with lengthening the terms of office (especially the 4-8-4: that is, electing the president, the House, and half the Senate—given eight-year terms—every four years); and (3) with replacing failed governments. He then discusses lesser proposals for fostering interbranch collaboration and altering the checks and balances (including incisive critiques of the presidential item veto and of the possible rehabilitation of the congressional veto devastated by the *Chadha* decision).

In the bicentennial, this book should inform the celebration of the Constitution by insisting on a thorough reexamination of its present adequacy. I dissent, however, from Sundquist's espousal of incrementalism:

> The parliamentary system represents only a source of ideas for incremental steps that might bring more unity to the American government, each such step to be considered on its own merits in terms of its adaptability to American traditions and institutions. (p. 15)

Normally, incrementalism is the proper approach to change, but in 1787 the framers radically departed from incrementalism, thereby creating a system which, if it is to be changed fundamentally, has to be approached as comprehensively as the framers did. Charles H. McIlwain, who capped his illustrious career with *Constitutionalism: Ancient and Modern* (Ithaca, New York: Cornell University Press, 1958), writes, "Among all the modern fallacies that have obscured the true teachings of constitutional history, few are worse than the *extreme doctrine of the separation of powers*." He could find little historical background for the theory, "except the fancies of eighteenth-century doctrinaires." Excoriating the separation of powers for dissipating government, McIlwain stressed its departure from incrementalism: "Unlike the legal limitations in our bill of rights, it is not the matured result of centuries of trial and error."

Consider the 4-8-4 proposal as an incremental step. This might *(a)* consolidate elections so that voters could "learn by doing" that they are creating a government *and* an opposition, and *(b)* give the government enough time to develop and apply its program. However, it would not cope with failed governments for which a vote of "no confidence" is the obvious solution. Yet if we favor keeping a popularly-elected president to help insure a unified executive and a governing majority, can the "people's choice" be voted down by legislators chosen in single-member districts? Can this dilemma be resolved by educating voters to perceive the president as a leader of a governing majority rather than as the embodiment of the sovereign people? Another problem: Both history and logic tell us that only one house of the legislature can properly be vested with the vote of confidence—What to do about the Senate? Meanwhile, the vote of confidence alone would unbalance the Constitution unless paralleled by vesting the power to dissolve the government in the executive. Careful readers will note that this is not a plea for simple adoption of the parliamentary system, but rather an argument that constitutional reform is ineluctably complex. It cannot be properly approached piecemeal.

History shows the actual drafting of viable constitutions to be the work of relatively few people. Our vigorous democracy would stipulate that this small group be surrounded, informed, and interpreted by Carl Friedrich's "constituent group." Political science can lead in the fearful task of forming the constituent group by focusing intensive study and debate on the Constitution. Can we, however, sustain such prolonged national questioning without undermining that reverence necessary to the maintenance of any regime? Plato's nocturnal council (*Laws*, 10) haunts us. Yet we may be able to have our inquiry and still hold fast to our devotion by distinguishing the essence of constitutionalism from the particular structures devised to implement it. Again, McIlwain is a good place to start.

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The Great American Crime Myth. By Kevin N. Wright. (Westport, CT: Greenwood Press, 1985. Pp. x + 227. $29.95.)

Crime is escalating, and the chances of most Americans being victimized by a violent criminal are extremely high. The fear of crime is a rational response by those people who are the most vulnerable to crime. To combat the crime problem, we need a no-nonsense, punitive strategy, and must allocate even greater resources to the agencies of the criminal justice system. Are these propositions factually correct?

Based on a thorough synthesis of the empirical literature on crime, victimization, fear of crime, and the operational capacities of the criminal justice system, Kevin N. Wright concludes correctly that these propositions are among the 11 myths about crime and criminal justice in America (see p. 9 for a full listing of the myths). Wright's lucid debunking of these myths is coupled with a straightforward explanation of the realities of crime and criminal justice in America.

For example, only 1 in 100,000 Americans are murdered by strangers, and less than 2 people in 1,000 are robbed by strangers and seriously injured. Wright points out that, compared with other life-threatening situations, a citizen's life is safe from victimization by a violent stranger: "You are more likely to die as a result of a motor vehicle crash (sixteen times), a fall (four times), drowning (two times), or a fire or burn (two times)" (p. 48).

When he looks at victimization data, Wright shows that those who are the most vulnerable and the most fearful of crime—the elderly and adult women—are among those groups least likely to experience victimization (pp. 57–62). In fact, like so many social problems, street crime is a dreadful threat to young, black, male inner-city residents. Despite broad-based fear of street crime, it is a targeted problem.

Why, then, is there such fear of crime among the broader public? Wright points to three sources: politicians' quest for easy votes; the American news media's competitive drive for subscribers and audience shares to boost advertising dollars; and criminal justice administrators' search for bigger budgets. These sources "exploit the public by sensationalizing crime" to pursue their organizational and self interests (p. 191).

Although crime is hyped and most citizens are unlikely to fall prey to a street criminal, this brand of crime is a grave threat to inner-city residents, particularly the young. Will a more punitive approach to crime and the allocation of more resources to the criminal justice establishment bring this threat under control? Wright points out that many of the "get-tough" strategies assume incorrectly that the only motivation for street crime is easy gain, and that increasing the costs of crime will lead people to refrain from such activity (p. 179). He also shows how difficult it is for the criminal justice establishment to implement strategies based on deterrence and incapacitation. For example, despite the enormous growth in resources allocated to law enforcement agencies, the certainty of detecting criminal behavior is far too low to make formal punitive measures the answer to America's crime problem (pp. 112–25).

While employing a different method, Wright arrives at the same conclusion reached by many critical criminologists who concentrate on the historical processes and structural forces that shape America's definitions and responses to crime. Developmental processes in the United States have eroded informal controls and increased the reliance on formal punitive controls. The dependency on formal sanctions and governmental agencies to enforce the social order will be of limited use in reducing the incidence of street crime in a heterogenous culture that openly promotes exploitation, material achievement, competition, and individual autonomy (pp. 181–202). For some in our society, unethical and illegal business practices evolve as a strategy for achieving these norms. For others, street crime, particularly property-related crime, serves as a vehicle for responding to powerful cultural cues. Wright sees this as inevitable for the predictable future, and claims that the criminal justice system can do no more than it is already doing to bolster the social order.

Is Wright correct in concluding that we are achieving all that we can with the formal criminal justice system? Closer analysis of the inner workings of criminal justice institutions indicates that we can do more to improve the work environment for thousands of street-level employees, who can make a difference in handling the clients of the criminal justice system. Furthermore, much more can be achieved to reduce the tensions between inner-city residents and the agencies of criminal justice.

Also, Wright fails to consider the benefits of reducing our society's heavy reliance on the criminal justice system and other formal punitive methods for managing social problems. A number of "crime" problems require broader definitions and the redistribution of resources to enable government to play a positive role. Even criminal justice administrators are coming around to this position with regard to the drug problem in America. With hundreds of millions of dollars added to the budgets of law enforcement agencies in recent years to eliminate the drug problem and few results, some criminal justice administrators are openly concerned about taking "a problem that is a medical, social and law enforcement problem, and holding law enforcement responsible for solving the whole problem by itself" (Leslie Maitland, "President's Antidrug Task Forces Are Falling Behind in Organizing," *New York Times*, May 1, 1983, p. 1).

Kevin Wright's book represents an excellent source for framing classroom discussion of criminal justice policy in the U.S. By combining his work with the conservative writings of James Q. Wilson, who argues that punitive measures work and criminal justice agencies are capable of exercising even greater control over the crime problem, students enrolling in the growing number of political science courses devoted to the living law and/or criminal justice would be exposed to the major, contrasting views on these topics.

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COMPARATIVE POLITICS

Politics in Israel. The Second Generation. By Asher Arian. (Chatham, NJ: Chatham House Publishers, Inc., 1985. Pp. xii + 290. $25.00.)

What Makes Israel Tick? How Domestic Policy-Makers Cope with Constraints. By Ira Sharkansky. (Chicago: Nelson Hall, 1985. Pp. xii + 184. $22.95, cloth; $9.95, paper.)

Israel: The Partitioned State. A Political History Since 1900. By Amos Perlmutter. (New York: Charles Scribner's Sons, 1985. Pp. xiv + 398. $19.95.)

Here are another three books about Israel, and as always, the question is, Why do we need so many? I suppose there is some happy conjunction between authors and publishers. All three of the authors under review are well known in the United States, and their collected corpus of scholarly output is impressive indeed. All three have American educations, residences, and experiences, and all have lived in and worked in Israel. Their credentials are high. Nevertheless, the three books are not of equal quality or value.

Arian's *Politics in Israel* is probably the best book on Israeli domestic politics that I have read out of several hundred over a period of 38 years. A quick perusal reveals how little the average American with a real interest in Israel knows about the intricacies of politics there. A close reading makes some subtleties apparent that are difficult to glean from the average American university course (or text) on the Middle East.

As Arian points out, "Political scientists who compare political systems find difficulty in fitting Israel into their schema" (p. 1). Israel is unique in so many ways. It is a democracy, yet like no other in the world. Certainly it does not resemble that of the United States. It has political parties, but none has ever won an election as we understand the term. Herut is a party; it is also the core of Likud, which is an electoral coalition. In order to govern, Likud is forced into a government coalition. Parties and their coalitions are tied to other institutions; the relationship between the Labor party (and coalition) with the "associational interest group" Histadrut is particularly interesting and profoundly important. The result is that every politician in Israel plays a variety of roles—bureaucratic, associational, ideological, and parliamentarian. Burdened with a miserable electoral system (characterized by list-system public relations), a growing ethnic division between Sephardim and Ashekenazim, generational gaps, perennial uncertainties among the electorate, and an ubiquitous military presence, it is a wonder that any Israeli government can survive and accomplish policy objectives. A close examination of the daily crises of the Peres government (itself certainly a peculiar variation of party democracy) should be revealing enough.

Arian's book explains much of this and more. I am most impressed with his analysis of Israeli democracy as an institution, and of the labyrinthine connections and interconnections of oligarchical and hierarchical organizations with party posturing and success. It is an enormously complex system understood by too few outside Israel. Arian's *Politics in Israel* is a superb book, masterly crafted.

Sharkansky's *What Makes Israel Tick?* (what an unfortunate title) is a useful complement to Arian's book. There is not that much overlap. Sharkansky defines the focus of the book as "policymaking for domestic activities" (p. 4), and in general he keeps to this task very well. The book is occasionally marred by such enthusiastic clichés as the following: "The desert has been made to bloom. Forests have taken root on slopes made barren by earlier inhabitants" (p. 5). It takes nothing from Israeli achievements to acknowledge that deserts bloomed in Palestine before there were Israelis there. Nonetheless, the book makes a positive contribution to our understanding of such disparate subjects as how Jerusalem is governed and how budgets—such as they are—are put together and manipulated.

Although they constitute only a small part of the book, Sharkansky cannot resist sharing with us his views on "warfare" and welfare (see chap. 9). Although these topics do relate to policymaking, and certainly affect domestic politics, the strength of Sharkansky's book, like Arian's, lies in its analysis of how the system works (yes, how it ticks), rather than in verbiage about Israel's defense posture, or peace prospects . . . or wars. Nevertheless, this is a good book—a quality book, as its publisher suggests on the cover.

I really have the least to say about Perlmutter's *Israel: The Partitioned State.* Perhaps I can begin by carping about the title. It suggests that Israel is a state that has been partitioned and that somehow it has been in existence since at least 1900. Since its creation in May 1948, Israel has not endured any partitioning, although Palestine *as an area* has. However, Israel and Palestine are not the same. To suggest that Israel had a history in 1900 is a little absurd. What had a history in the early years was Zionism due to a growing population of Zionist settlers (Yishuv) in Palestine and their relations with a British mandatory administration. These things in the title suggest the orientation of the book. The history of Israel (and of Zionism) and its wars with the Arabs has been written about so exhaustively—and by Perlmutter, for that matter, in other times—that there is no crying need for this book. It does not illuminate as many dark and interesting corners as do both Sharkansky's and Arian's books.

Few books are without value, and Perlmutter's is no exception. If Perlmutter has a specialty, it is the military and its games (Arab as well as Israeli). His views on subjects like these are noteworthy, even if one disagrees with details. His discussion of the Begin period is very good. Indeed, in my opinion, the real value of the book begins with section 4, which treats the period from 1973 to the present. I might add that of the three books, Perlmutter's is certainly the most colorfully written, and consequently, the easiest to read. Yet, I can only admonish readers of this book to exercize critical judgment as they are swept along by its imagery and kaleidoscopic metaphors. Many of Perlmutter's views lend themselves to alternative interpretations, and this work is hardly definitive.

All in all, one very good book, one good book, and one not-bad one can be added to the bulging shelf of books on Israel.

CARL LEIDEN

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Two Political Worlds: Parties and Voting in British Columbia. By Donald E. Blake. (Vancouver: University of British Columbia Press, 1985. Pp. x + 205. $15.95.)

Donald Blake has written an exceptional book on the dual nature of the party system of Canada's most western province, British Columbia. Within this province's two-party system, Canada's two major national parties, the Liberals and the Progressive Conservatives, achieve almost no electoral success. The party system at the federal level includes three parties: the two major parties and the New Democratic Party, which is a principal participant in the provincial electoral context. To explore this phenomenon, the book

> presents a detailed look at the inhabitants of these two political worlds. It traces the evolution of the two party systems, seeks to identify the dimensions of conflict which structure them, and analyses the behaviour of voters who participate in them. (p. 1)

The book has several significant virtues. At the outset it can be noted that the empirical findings derived from "the first [1979] comprehensive survey of the political attitudes and behaviour of British Columbia voters" (p. ix), supplemented by survey data from national election studies, are integrated within a sound theoretical and contextual framework. This analysis does not ignore the importance of the historical sociopolitical milieu within which political behavior functions. Additionally, the findings deal with three significant questions pertaining to Canadian voting behavior: class voting, western alienation, and divided partisanship. The analysis of class voting goes beyond "a simple dichotomous explanation" (p. 111), to a more sophisticated treatment of class that recognizes the impact of the workplace and its environment in shaping the behavior and attitudes of individual voters. The common assumption that British Columbia's divided loyalties in voting behavior are the result of western alienation is challenged, and that "alienation is probably an inappropriate label" and that better words would be "discontent, policy grievances, or partisan rivalry" (p. 133) may be more appropriate, adds not only to a better understanding of British Columbia politics, but also to a better understanding of nature of regionalism within the Canadian federal political system. Finally, the book's major assertion—that the divided loyalty of British Columbia voters is not the result of "instability" in party identification, but rather the consequence of divergent "structural, institutional and strategic bases" (p. 173)—is a breath of fresh air that helps us comprehend why voters switch parties between federal and provincial elections.

The weakest part of the book deals with the question of alienation. While Blake examines several dimensions of what he refers to as *alienation*, he never defines the concept precisely. Words such as *allegiance, discontent,* and *grievances* are used interchangeably when referring to alienation. This lack of conceptual clarity detracts greatly from Blake's findings on the role of alienation in the province of British Columbia.

All in all, however, this concise book is a first-rate piece of research. It will serve as the model for future studies of the two political worlds of British Columbia politics.

ROBERT KRAUSE

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Political Change and Underdevelopment. By Vicky Randall and Robin Theobald. (Durham, NC: Duke University Press, 1985. Pp. ix + 219. $30.00, cloth; $11.95, paper.)

Third World Politics: An Introduction. By Christopher Clapham. (Madison: Wisconsin University Press, 1985. Pp. 197. $25.00, cloth; $12.50, paper.)

These two concise volumes both offer assessments of the state of knowledge about politics in the diverse "Third World." Randall and Theobald offer a survey and critique of major theories of political change in Asia, Africa, and Latin America. Clapham offers his own integrative interpretation of the nature of the state and its implications for politics in the Third World. Both books are remarkably wide ranging within a brief span of roughly two hundred pages. Both share an eclectic, well-balanced, integrative spirit that takes account of, but is not bound by, various theoretical approaches.

Randall and Theobald's survey begins with the "modernization" theories of the 1950s and 1960s, with their teleological and dualist assumptions. The authors then review the major early challenges to these assumptions: the discovery of the durability and flexibility of "tradition" and the need to give more explicit attention to the requisites for establishing and maintaining political order. A discussion of dependency theory then expands into a consideration of more recent neo-Marxist and related non-Marxist political economy approaches. Interspersed with capsule accounts of major contributors to each of these schools of thought are illustrative applications to specific cases. Throughout, the authors offer their assessments of the strengths and weaknesses of each approach.

Inevitably, informed readers will take exception to some specific judgments or interpretations. For instance, I disagree with the assertion that those analysts of the late 1960s and early 1970s who focused on the problem of political order were also admirers of military government and ignored the risk of the abuse of state power. In general, however, Randall and Theobald do an admirable job of presenting and assessing the evolution of thinking about politics in the Third World over the past 35 years.

Clapham's slim volume, in contrast to Randall and Theobald's, does not seek to assess others' theories. Nor, by his own account, does he attempt to formulate his own theory. Rather, his goal is to provide "a sense of how things fit together" (p. 9)—that is, the common strands in the complex and varied kaleidoscope of Third World politics. He focuses on the nature of the state, and argues that the feature most clearly distinguishing Third World states is the weakness of shared social values and public goals that would provide a legitimate purpose for state power. As a result, the state and its "owners" rule largely in their own interests, and the state, being illegitimate, is fragile. The absence of unifying goals reflects the artificial character of most Third World nations, as do the effects of past (colonial and quasi-colonial) and current penetration by external forces. In the absence of legitimate state goals, neopatrimonialism becomes the main mechanism for regimes to maintain support. Clapham traces the ramifications of this analysis for the management of the economy, external political relationships, internal political-military relations, and revolutionary tendencies (or the lack thereof).

The book is really an interpretive essay, incorporating many of the themes of recent and not-so-recent analysis, and sprinkled with neatly formulated insights. Conceptually, its major weakness seems to me the failure to consider the strategies available to governments (other than variations on the neopatrimonial theme) for strengthening national coherence and building a consensus on legitimate goals and functions of the state. Clapham's discussion of strategies available to ruling coalitions centers wholly on various groups within the population and on different styles and tactics available to rulers, ignoring the content of policy. The end result is a pessimistic and static aura that, while it captures part of reality, omits other crucial aspects.

Both books are intended as teaching tools. Randall and Theobald succeed more fully, in the sense that their volume can stand on its own, and will be valuable at the introductory and intermediate levels. Clapham's volume is subtitled, *An Introduction*, but it might in fact be difficult for those with little prior knowledge of Third World politics to grasp. In contrast to Randall and Theobald's illustrative case materials, often developed over several pages, Clapham simply refers to cases—a technique meaningful only to those who already are familiar with the cases. Used in combination with more detailed case studies, however, this book could be an excellent teaching tool.

Randall and Theobald suggest, in closing, an agenda for research that bears on the weak point in Clapham's discussion. Both volumes stress the authoritarian nature of most Third World states, and both are thoroughly skeptical about the mechanisms earlier theories thought would produce either democratic evolution or revolution. However, these are not the only possible patterns of progressive change, in the sense of movement toward closer and less exploitative integration between state and society. Randall and Theobald call for research that would distinguish different patterns of authoritarianism and seek to discern conditions and paths of change, particularly progressive change. That agenda grows out of, and should be informed by, the various approaches their book reviews, but it redirects attention to reflect more fully the realities of much of the Third World. I do not regard it as ethnocentric to argue that such an approach may also better serve democratic values.

JOAN M. NELSON

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The Political Dilemmas of Military Regimes. Edited by Christopher Clapham and George Philip. (Totowa, NJ: Barnes & Noble Books, 1985. Pp. 282. $28.50.)

In addition to restoring law and order after a crisis situation, military regimes, once in power, face the added burden of having to remedy societal ills. Military regimes are confronted, however, with the conflict between their own requirements and the needs of a given society. As the authors writing in this edited volume ask, What can military regimes do with their power once in office?

Clapham and Philip set out to identify the political dilemmas facing military regimes. Both authors assert that the choice, although not unique to such governments, is between institutionalization and demilitarization. Types of military governments are classified into four groups: *veto*, *moderator*, *factional*, and *breakthrough* regimes. This concise theoretical discussion is useful, since it leaves the reader with a solid framework for understanding the role of power and how it is exercised by military regimes. However, the editors also acknowledge that the political conduct of military regimes is more a matter of "circumstances" than deliberation, given their desire to cushion the military from civil society (p. 24).

In his discussion of the veto regime in Greece between 1967 and 1974, Thanos Veremis illustrates the military's dilemma quite well. Seven years in office had created a power vacuum, and the military collapsed under its own weight because it did not know what to do with organized civilians. Bener Karakartal's study of the Turkish military, on the other hand, reveals the existence of a high degree of internal unity, in that the military command structure was differentiated from civil society. In Ankara's case, the military acted as a moderator influence, encouraging the return to civilian rule as soon as possible. Yet, the election of Prime Minister Ozal, while supporting "the longstanding separation in Turkish political culture between the military and civilian parties" (p. 60), did not mean the elimination of the military's influence, a role more comfortably played behind the scenes.

Authoritarianism in the military is also discussed by Guillermo Makin, who asserts that junta members in Argentina have used varying methods to achieve varying goals with varying results. This experimentation and the lack of progress may have forced Buenos Aires to revert to civilian rule. Moreover, the election of Alfonsin to the presidency was expedited by numerous military blunders, creating the impression of power where none existed. Nevertheless, whereas the South American cases (an overview is presented by George Philip) indicate that relative independence does exist between society and state, this is not the case in Central America. James Dunkerley argues that the military's overall inability to accomplish even minimum political objectives has resulted in failures whose outcomes remain obscure. Indeed, unfolding and bloody Central American revolutions stand in testimony to corrupt and purposeless military regimes.

In contrast to the Central American cases, the sense of purpose is very much evident in several Asiatic states, where the military strives to maintain internal unity. Ulf Sundhaussen and Barry R. Green assert that the Indonesian regime stands as a relative success story, since it "appears to be intent on hanging on to [power] at all costs" (p. 103). Soeharto is credited with an improved economy but, perhaps more importantly, with the political savvy of persuading minority religious parties to abandon any political ambitions. Alternatively, the Soeharto regime has succeeded by offering more political rights to most people, "provided they [were] neither communists, Islamic fundamentalists, nor separatists" (p. 109). Indonesia still faces an uncertain future, given the absence of a strong successor able to take over from Soeharto.

Gowher Rizvi's study of Pakistan and Bangladesh discusses how the military in both countries found itself in an untenable position, having alienated the principal civilian organizations and created a clientelist relationship with various groups. Nevertheless, the power of the masses never eroded, leading a figure like General Zia ul-Haq to accept an eventual return to civilian rule. In fact, the recent restoration of civil rights in Pakistan further illustrates the degree to which power remains elusive.

By far the two most interesting studies in this book are those of the breakthrough regimes in Ethiopia (Christopher Clapham) and the Sudan (Peter Woodward). In Ethiopia, the difficult path pursued by the state has led to civil and economic disasters. In the Sudan, factional clientelism has kept the Numairi regime in power for over 15 years. Yet, as Numairi recently discovered, real power has always rested with the military, not fledgling civilian institutions, and his refusal to acknowledge this source of ultimate power led to his removal from office.

This is an important study of several military "establishments" where contrasting values of power guarantee successive coups, whether violent or not. Readers will learn a great deal more than the theoretical framework, since the factual discussions are colorful and discriminating.

JOSEPH A. KECHICHIAN

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Electoral Change in Western Democracies: Patterns and Sources of Electoral Volatility. Edited by Ivor Crewe and David Denver. (New York: St. Martin's Press, 1985. Pp. iii + 438. $29.95.)

After a long period of stability, western nations seem to have been experiencing substantial electoral change in recent years. This text focuses on a salient characteristic of the change: the volatility of western electorates, defined principally as interelection shifts in the vote. The book contains 13 case studies—of the United States, Canada, Australia, Great Britain, Scotland, Ireland, France, West Germany, Austria, Italy, Belgium, The Netherlands, and Denmark—written by leading national specialists, plus an introduction and a conclusion prepared by the editors.

The principal strength of *Electoral Change in Western Democracies* is that it draws together under one cover a set of national case studies of uniformly high quality. Some of them, such as the excellent summary of electoral politics in Britain by Ivor Crewe, cover familiar ground. Others present new data and interpretations regarding electoral change. Particularly noteworthy among the latter are the chapter by Marsh on the stability of Irish voting patterns; Haepfer's analysis of the emergence of a small candidate-centered floating vote, located among well educated groups, in Austria; Allum and Mannheimer's demonstration of the attenuation of the social and communist/anti-communist base of Italian voting; Van Der Eijk and Niemoller's analysis of the growth of ideological voting in The Netherlands; and Borre's development of a model of fragmentation/realignment for Denmark.

A second strength of this volume derives from the common framework developed by the editors to guide the preparation of the national case studies. Each country's chapter addresses changes in electoral volatility since 1945 (employing the Pedersen measure of net volatility); explores sociological, ideological, and government/party performance explanations for these changes; and considers the relationship between electoral volatility and the party system. In the few cases where the appropriate individual-level data are available, gross volatility (i.e., individual vote change) is also examined.

The organization of each national study within a common framework promotes cross-national comparisons and gives this volume great potential for uncovering whatever general patterns may exist in processes of electoral change throughout the western world. The primary generalization, however, is that the western democracies have responded to substantial social change in diverse ways; i.e., no predominant electoral pattern has emerged. Some electorates have exhibited increasing levels of electoral volatility, but others have shown striking vote stability in the face of significant societal change. The common denominator of electoral change instead is what has been lost rather than gained; there is a weakening of the traditional social bases of their party systems as reflected mainly in the declining relationship between social class or religious identification and the vote.

That the potential offered by a common framework is not more fully realized may be attributable to the particular framework employed and not the inherent uniqueness of each nation. Electoral volatility is not a highly useful concept for summarizing electoral patterns. It is only one aspect of electoral change and probably not the central one because of its sensitivity to electoral arrangements as well as to voter behavior. More importantly, electoral volatility has *not* been increasing in a majority of the nations selected for study. The common framework forces many of the authors into the awkward position of trying to explain a nonevent and inhibits the pursuit of more profitable lines of analysis.

The operationalization of volatility also is troublesome. The aggregative Pedersen index is a poor measure of electoral change, especially in multiparty systems (see, e.g., Grunberg on France, Mughan on Belgium, and Van Der Eijk and Niemoller on The Netherlands). This weakness led the editors to include gross volatility in their framework, but the absence of individual-change data limits its application to only a small number of elections—and most of them only by using recall data.

Nonetheless, in spite of limitations in its overarching conceptual framework, *Electoral Change in Western Democracies* is a valuable contribution to the study of comparative politics. The relationships between societal change and citizen-voting behavior are complex in any single nation, and the case studies in this volume clarify these relationships well for their particular country. By adopting a common theoretical approach to each case, moreover, this volume advances our understanding of how voters in different electoral contexts respond to similar forces for change—a considerable achievement in a world that inevitably contains more causal factors than observations.

PAUL ALLEN BECK

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The New Select Committees: A Study of the 1979 Reforms. Edited by Gavin Drewry. (Oxford: Clarendon Press, 1985. Pp. xiv + 410. $29.95.)

The British Parliament of 1979–83 was described by one leading member as a "reform Parliament." Under pressure from backbenchers on both sides of the House, significant structural and procedural changes were enacted. Foremost among these was the appointment of 14 investigative select committees, set up "to examine the expenditure, administration and policy of the principal government departments . . . and associated public bodies." The committees have established themselves as major features of the parliamentary landscape. Yet what political impact have they had? The question is one much to the fore in the discussion of parliamentary efficacy and reform. It is a question that Gavin Drewry and other members of the British Study of Parliament Group seek to answer in this substantial volume.

The book constitutes a comprehensive study of the committees in the first parliament of their existence. There is a chapter on each of the 14 committees, plus two introductory and five analytical concluding chapters. There is also a chapter on committees in the House of Lords, though the rationale for its inclusion—given the absence of chapters on other Commons' select committees, such as the Public Accounts Committee—is not clear.

The chapters on the individual committees adopt a broadly similar format, covering membership, inquiries, mode of operation, reports, and impact. Each is clear and readable, incorporating much useful factual data. They nonetheless fall foul of two inherent difficulties. One results from the constraints of space. Each chapter has, of necessity, been kept short. Contributors are consequently unable to provide little more than factual summaries. There are no sustained case studies and little anecdotal material that would allow the reader to get the feel of the committees' work. Constraints of space also appear to account for some important features and problems of particular committees being skimmed over or not even mentioned. (The chapter on the Foreign Affairs Committee fails to mention the controversy engendered by Conservative disquiet over the operation of its Overseas Development Sub-Committee; only the editor, in a later chapter, touches upon it.) The second problem arises from the nature of the committees themselves. They are multifunctional bodies. From the beginning, they have differed significantly in the emphasis given to particular functions. Many of these functions entail seeking to exert "influence." How can one empirically verify what influence has been exerted? In most cases it is near impossible. The contributors have to fall back on generalized and intuitive assessments of the committees' impact, some doing so in such a generalized way as to be not terribly helpful to the reader.

The concluding chapters constitute the best part of the book. In the penultimate chapter, Philip Giddings skillfully draws out the multifunctional nature of the committees and places the attempt to fulfill those functions in the context of the wider parliamentary and political environment. He argues that the committees are most effective in ensuring greater "accountability" on the part of government: ministers and officials are sensitive to the fact that what they do may be the subject of committee investigation. This point is reinforced by the editor in the final chapter. Both writers argue convincingly that the committees more than justify their cost. Like the institution of which they are a part, the new committees provide good value for money.

PHILIP NORTON

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The Future of the Soviet Economic Planning System. By David A. Dyker. (Armonk, NY: M. E. Sharpe, Inc., 1985. Pp. 172. $25.00.)

The Soviet economy has increasingly been plagued by shortcomings and inefficiency in the ongoing process of shifting from an extensive to an intensive mode of production. David A. Dyker convincingly explains the seemingly inherent weaknesses of Soviet economic planning, which often result in lower-than-expected output targets. *The Future of the Soviet Economic Planning System* is a short, but highly sophisticated and concise treatise on the historical evolution, practice, and future prospects of centralized planning in the Soviet economy.

Stalin's economic growth strategy of output maximization, though necessary perhaps to achieve rapid industrial growth, resulted in waste of capital because of "gigantomania"; overbidding for investments in order to keep resources flowing; and the tendency toward organizational autarky because of supply uncertainty. Moreover, Stalin's approach to economic planning firmly implanted two key principles in Soviet planning tactics whose basic premises appear still to be at work today, despite a number of reform attempts. The "ratchet" principle implies that central authorities establish their plan targets by extrapolating from past input and output trends because of insufficient or untrustworthy reports on enterprises' production capacity. This in turn induces enterprise directors to try to achieve even but very modest plan fulfillments. The "Micawber" principle is an element of the crude growth maximization pattern by rewarding plan overfulfillment and punishing even the slightest underfulfillment.

Attempts at reforming the planning system began shortly after Stalin's death. Unlike Khrushchev's "peacemeal, disorganised and disorganising" changes (p. 42), however, only Kosygin's 1965 reforms did result in higher capital productivity, even though labor productivity remained stagnant. Most reforms appear to have failed because the overall system of Soviet planning has remained highly centralized, despite some "marketization" in peripheral sectors, and the principle of *khozrashchet* (business accounting) has only been introduced in low-level enterprises and not in intermediate planning bodies. Furthermore, events in Czechoslovakia alerted Soviet leaders to the political dangers of radical economic reforms, and the tremendous increase in oil, gold, and timber prices sparked the (largely false) hope that imports of Western technology could somehow substitute for economic reform. The post-Brezhnev leadership may, however, have the greatest difficulties in coming to grips with the underlying domestic problem of over-centralization, even though Soviet academics appear to recognize and acknowledge its detrimental effects.

The Soviet economic planning system has been plagued with a high degree of inertia that makes it virtually immune to any radical planning reforms. Three factors in particular impose barriers to fundamental change. First, ideological conservatism among leaders results in the pursuit of quantitative, computational improvements of centralized planning tasks. Second, a powerful party apparatus at the local and intermediate levels is extremely suspicious of, if not hostile towards, any autonomization of the economy. Third, the military sector has tremendously benefited from the "priority principle," whose very existence dictates a command economy.

The analysis by David A. Dyker and the well-documented evidence of weaknesses and shortcomings in the Soviet economic planning system pose a powerful argument against any high expectations of radical and rapid reforms. The only pitfall this study may contain is an underestimation of Soviet economic achievements within a relatively brief time period, and in the face of numerous setbacks that wreaked havoc upon the existing industrial production capacities. Ignoring Soviet economic successes could, in turn, lead to the erroneous assumption that radical reforms towards "liberalization" are the *sine qua non* for the system's survival. The leadership may well continue to "muddle through," particularly if we keep in mind that the economic planning system has shown a tremendous tenacity in the face of external and internal threats.

Nonetheless, Gorbachev's avowed aim of cracking down on inertia and political privileges and the actions taken so far must be regarded as a serious challenge to the existing system of economic planning and policy implementation. *The Future of the Soviet Economic Planning System* will serve as a benchmark against which the success of Gorbachev's reform movement may be evaluated.

BEAT KERNEN

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Politics Against Markets: The Social Democratic Road to Power. By Gøsta Esping-Andersen. (Princeton: Princeton University Press, 1985. Pp. xx + 366. $32.50.)

Why have working-class, political parties been so successful in Scandinavia? Why have they suffered stagnation or decline in popular support during the past two decades? What, if anything, can be done to rebuild support among changing national constituencies? These are analytically interesting questions, but they are also vitally important to the future of the welfare state. Gøsta Esping-Andersen's new book, *Politics Against Markets*, offers answers that are at once thoughtful and thought provoking. Along the way, he provides a sophisticated historical comparison of relationships among class structure, party systems, and public policies in Denmark, Norway and Sweden. Anyone interested in the future politics of welfare states will find Esping-Andersen's argument both rich and rewarding.

In Esping-Andersen's view, most theories that seek to account for the development of working class parties have "concentrated almost entirely on class structural change, in the belief that the logic of labor movements springs from the nature of the social structure" (p. 4). Esping-Andersen, however, believes such theories to be entirely too deterministic and offers an alternative, which is that the development of social democratic parties "depends on the impact of state policies on the class structure" (p. 26). Although the occupational structure in each nation obviously sets parameters for the possible, policy choices made by governments controlled or influenced by working-class parties can either promote or retard the continued development of such parties. In all three countries, for example, the existence of an independent and politically aware peasantry was an important constraint against development of a unified conservative opposition. More importantly still, farmers and peasants were willing to form alliances with urban social democrats in the 1930s in order to secure economic benefits for themselves. These worker-peasant alliances provided firm political foundations for three decades of welfare state initiatives that, in reforming government social policies, stimulated growing political support for Scandinavian social democrats.

To document his argument, Esping-Andersen leads the reader through three areas of policy development in each nation: social assistance, housing, and macro-economic policy. Valuable as sources of information, these chapters have the additional virtue of demonstrating the considerable political differences between these nordic neighbors. Denmark, where agriculture and skilled crafts remained dominant until the 1960s, moved less rapidly toward universal entitlement programs, pursued housing policies that divided renters and owners, and used tax and incomes policies that maintained, rather than reduced, income inequalities. Under these conditions, bourgeois party support grew and left-wing social democrats withdrew to splinter parties.

In Norway, social democrat-bourgeois consensus on welfare state programs was fueled by solidaristic housing policies that offered all citizens an opportunity for government loans at favorable rates and by massive government participation in credit markets. Sweden provided the model for transition from a workers' to a peoples' party. Thus it led the way in institutionalized entitlements for all citizens, renewed the national housing stock while offering subsidies to renters that matched tax breaks available to home owners, and developed an "active labor market policy" that encouraged both labor mobility and greater productive efficiency. Social Democrats in both Denmark and Norway were badly split by the European Economic Community (EEC) issue. The Norwegian party, buttressed by solidaristic housing and economic planning policies, was able to recover to some extent while the Danish party has continued to struggle. Swedish social democrats, on the other hand, continue to enjoy support among all population segments, although support has not increased in the past decade.

Documented by painstaking historical research, Esping-Andersen's argument that government policies had decisive effects on social democratic support persuasively challenges more simplistic theories of class causation. His analysis of the present dilemmas of Scandinavian social democracy is equally persuasive. The institutionalized entitlements of Nordic welfare state programs are not only expensive but, the more universal they become, the greater the need to raise additional tax resources to fund them. Since middle-income taxpayers provide most of the tax revenues in each nation, their support is essential for improved welfare programs. As taxes increase, however, middle-class support declines, unless social democrats are able to devise effective programmatic responses. Swedish social democrats found such a response in their pension supplement program of 1959, but the Norwegians have had less success, and the Danish party has had almost no success at all. The future of social democracy requires alliances with middle-income taxpayers, but traditional social democratic policies appear to render such coalitions impossible.

Is there a solution to the social democratic dilemma? Absent any other ideas, Esping-Andersen suggests that the Swedish proposal for "economic democracy" is the key to party renewal. Developed largely by Landsorganisationen (LO), the central labor confederation, the Swedish proposal for wage-earner investment funds has the potential to structure a new coalition between blue-collar and white-collar workers. For labor, ownership shares in corporate profits could dampen pressure for higher and higher wages, thus relieving social democratic governments of the difficult problem of imposing wage restraints on their strongest supporters. For middle-income taxpayers, the availability of large funds for capital investment could provide a resource to maintain productivity and growth, thus relieving them of the burden of ever-increasing taxes. For the nation as a whole, implementation of the plan could provide more effective tools for macro-economic planning to complement worker-participation schemes already in place. Together, these benefits could secure an electoral base for the social democrats into the foreseeable future. Esping-Andersen acknowledges that the plan has little support beyond the LO membership and strong opposition from the bourgeois parties in Sweden. Nevertheless, he regards the wage-earner fund proposal as "the only existing plan that permits labor to escape from the economic dilemma in which it finds itself" (p. 310).

Although Esping-Andersen is clearly committed to the rejuvenation of social democratic parties in Scandinavia, it is not clear why. In Norway, for example, welfare state programs were a product of consensus among the social democrats and their bourgeois party colleagues, while the Swedish bourgeois parties continued to support such programs during their recent six-year (1976–82) hold on the government. If social policy is the metric, it is difficult to understand why one party should be preferred to others. Or, to take another example, figures reported by Esping-Andersen (pp. 239–41) show that per capita Gross Domestic Product (GDP) and real manufacturing wages grew much more rapidly in Denmark than in Sweden from 1960 to 1979, while inflation was only modestly higher in Denmark (10.7% vs. 10.4% from 1974 to 1979). If economic policy is the metric, strong social democratic control in Sweden was less effective than the market-oriented activities of Danish politicians. Unemployment has been considerably higher in Denmark than in either Norway or Sweden during the entire post-war period, of course, but Esping-Anderson does not say whether or not he believes full employment to be the supreme criterion of judgment. Indeed, the various performance measures he reports are so varied that use of a single evaluative criterion would make little sense. How, then, should we understand his obvious interest in social democratic renewal?

One way to appreciate the perspective Esping-Andersen brings to his study is to recognize alternative conceptualizations that he does not use. An alternative that comes to mind immediately is the "party systems" framework. Had he employed this framework, Esping-Andersen would have paid much more attention to the bourgeois and radical parties of Denmark, Norway, and Sweden, perhaps enriching his theory of policy-generated support by applying it within, as well as between, nations. Among other benefits, this framework might have enabled him to resolve such puzzles as the continued reluctance of the Swedish bourgeois parties to cooperate, rather than simply dismissing the issue as a matter of social democratic good fortune. However, Esping-Andersen focusses almost entirely on the labor parties, referring to others only when necessary to support his analyses of labor party behavior.

The focus on "labor parties" rather than "party systems" or some other formulation stems from Esping-Anderson's decision to cast his study as a continuation of arguments about socialism begun years ago by Marx, Lenin, Kautsky, Bernstein, and a host of other socialist theorists. Most of these arguments became irrelevant in Scandinavia rather quickly, as social democrats chose first to participate in capitalist governments, then to compromise in order to form majority coalitions (usually with farmer-peasants), and finally to pursue reformist rather than revolutionary goals—largely because voters repeatedly rejected party appeals for socialist purity. Given this historical record, perhaps it is not surprising that Esping-Andersen's effort to contribute to these old debates produces rather meager results, consisting largely of his rejection of the distinction between "reformist" and "revolutionary" goals in favor of an amended definition of social democracy as "a movement that seeks to build class unity and mobilize power via national legislation" (p. 10). The reference to national policy here seems quite sensible, but it is also apparent from his later chapters that use of this definition would exclude the Danish party for much of the past two decades, and partially exclude the other two parties as well. In short, Esping-Andersen's turgid struggle with the socialist cobweb seems neither productive nor clear, and quite unnecessary for the argument that follows in his text.

Nevertheless, it is a tribute to Esping-Andersen's integrity as a scholar that his analysis provides material to support theoretical interpretations quite different from his own. Voters who variously support social democrats depending on benefits offered and parties that seek majority coalitions by designing programs for different party constituencies both suggest a model of party pragmatism that is quite consistent with the data reported by Esping-Andersen. With voter and organizational self-interest at its core, in fact, such a model might help us to understand various bourgeois and splinter parties as well as the social democrats, and it could clarify prospects for party success in dealing with the new issues that Esping-Anderson believes will determine future voter alignments: job security and quality of working conditions. The pragmatic model is not developed in *Politics Against Markets*, obviously, but the data reported are rich enough to support this and perhaps other interpretations. Whether or not we accept Esping-Andersen's conclusions, therefore, all of us should be grateful for the care with which he develops his argument and the integrity with which he presents his data. *Politics Against Markets* is a powerful stimulus for productive thought about a central issue of our time.

THOMAS J. ANTON

Brown University

The Decline of Class Voting in Britain: Changes in the Basis of Electoral Choice, 1964–1983. By Mark N. Franklin. (Oxford: Clarendon Press, 1985. Pp. xii + 194. $34.50, cloth; $14.95, paper.)

Franklin's well-crafted and subtle book focuses on two questions: Has class voting in

Britain declined since the 1960s? If so, why and with what consequences? He answers the first with an emphatic "yes" and the second with an unemphatic "difficult to tell."

The study is the theoretical and empirical child of Butler and Stokes' seminal *Political Change in Britain.* Its departure point is a "rehabilitation" of Butler and Stokes' socialization model of partisanship. Franklin turns their six main social structural variables into a causal model of the vote, and reanalyzes the British Election Studies from 1964 to 1979 to trace their changing impact. He shows that their capacity to shape the vote declined markedly (if unsteadily) over the period and was replaced by a growth of issue voting. In an elaborate analysis he demonstrates that only half the decline arose from the changing shape of the class structure—in particular the growing proportion of the electorate with mixed class characteristics—and that almost none could be attributed to the 1974 surge of minor party voting.

Franklin's main thesis rests on the particularly marked decline in the impact of the "central class" variables of occupation and parental class, as distinct from the merely "supplementary" class variables of housing, trade union membership, education, and parental partisanship. The latter, Franklin argues, can transmit and sustain support for *any* party; only the former *necessarily* socialize the electorate into Conservative and Labour support. The collapse of their influence means that since 1970, the structure of class voting has lost its backbone and cannot be relied upon to sustain the two-party hegemony. Only inertia and accident enable the Conservative and Labour parties to continue precariously as major parties. The future is one of volatility and unprecedented electoral uncertainty; not for 60 years has the party system been so "ripe for change."

In the course of this argument, Franklin challenges the numerous competing accounts of electoral change that have recently appeared. Those that side with him in discerning the demise of class voting—e.g., Himmelweit, Whiteley, and Rose—are gently but effectively admonished for technical weaknesses. (One of the book's side benefits is an exceptionally clear description of the complexities of different statistical techniques.) Those who try to resurrect the primacy of social structure are given much shorter shrift. In a devastating section, Franklin demolishes consumption cleavage theory as conceptually vague, inadequately modeled, and lacking in empirical support. The ecological paradox of a strengthening of class influences at the constituency level is also dismissed—less plausibly—as an artifact of trends in the influence and areal distribution of housing.

In seeking explanations, Franklin singles out 1970 as the electoral turning point, but the early 1960s as the social starting point. The crucial change was not the "swinging sixties," pace Beer, or the "silent revolution," pace Inglehart, but the abolition of military conscription between 1958 and 1960. Innocence of the barrack-room class system combined with new-found affluence created a classless new generation within the working class, for whom politics was a matter of government performance, not class loyalty. In 1970 they swung massively against the conspicuously incompetent Labour government. Labour's electoral rot set in under Harold Wilson, not James Callaghan or Michael Foot. It is an original and intriguing argument, even if, as Franklin acknowledges, the evidence is incomplete.

Skeptics will quibble with Franklin's analysis here and there. The argument occasionally seems forced, resting on over-analyzed data or unduly speculative inferences. The dependence on respondents' recall of parental partisanship and class is troublesome, given the notorious unreliability of voters' recall of their own past partisanship. The argument that occupational socialization must prop up the Conservative and Labour parties, whereas housing socialization can sustain any party system, is not fully convincing. The absence of any reference to race as an element in the social structure and as an electoral issue (especially in 1970) is disappointing. Nonetheless, Franklin's contribution to the rapidly growing controversy on the role of class in British elections is the most sophisticated, painstaking, and, in my own view, plausible published so far. Anybody wishing to understand the complexities of the issues at stake will finish the book well rewarded.

IVOR CREWE

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Administrative Politics in British Government. By Andrew Gray and William I. Jenkins. (Sussex: Wheatsheaf Books, and New York: St. Martin's Press, 1985. Pp. x + 259. $26.00.)

Six years of the Thatcher Government, with its especially strong view of the proper relationship between elected and permanent officials, have given rise to an outpouring of interpretations of the state of British bureaucracy. Most of these works have focused on immediate questions and lack scholarly rigor, often centering on such hardy perennials of that system as government secrecy and the perverse uses of ministerial responsibility. Recently, the literature has gone in for mapping trends toward a more politicized civil service without giving that concept much specificity. One of the virtues of Gray and Jenkins' book is that they approach the alleged transformation of public administration in Britain from a fresher perspective than most other commentators. Their preoccupation is with organizational theory as a guide to what can be done to change the mainsprings of deeply entrenched attitudes and practices. Eschewing the more ponderous abstractions (and, happily, the terminology) of that genre, they employ a mixture of theoretical concepts, empirical references, and case studies to explore the influence of organization politics on the conduct of the central administration. Followers of Graham Allison will recognize familiar things here, although the authors insist that their version of bureaucratic politics takes in much more, notably, contextual elements that give rise to differing interests from department to department in "village Whitehall."

How well does the mixture work? (It is not really a "model," despite the authors' claims, but is not necessarily the worse for that.) When they can securely connect their well-honed concept of administrative politics with detailed government policy making, their analysis is quite persuasive. For example, they bring insight to the problem of engineering change in administrative outlooks by showing that those governments that take the trouble to understand the underlying organization, culture, and tasks of the diverse units making up the congeries called Whitehall stand a better chance of getting their way than those that opt for an undifferentiated blueprint for change. In the search for ways to import managerial efficiency into the civil service, reforms of the former sort—characterized by the Rayner scrutinies and Heseltine's Management Information System for Ministers (MINIS)—have had impact and shown staying power, while more sweeping programs—such as the Fulton Committee's ideas and Programme Analysis and Review (PAR)—have been sidetracked rather easily by experienced players of administrative politics. Gray and Jenkins' discussion of such matters puts into perspective Thatcher's obsession with enhancing management at the expense of policy. On the other hand, in those cases where the behavior of politicians and bureaucrats can be fitted into the authors' theoretical framework only tangentially, as in their case study of the Falklands dispute, the result is more commonplace and provides little support for their overall approach. The book, then, like the structure of British administration, is quite variable, with strong chapters juxtaposed with pedestrian ones. Mercifully, when chapters are good they are very, very good, as is true of those on "The Politics of Efficiency" and "The Politics of Administrative Reform."

In part because the authors have theoretical aims and are concerned not to overuse case studies, their work has a "from the outside" flavor. Such an orientation has costs for those who stake much of their argument on the twin concepts of administrative culture and bureaucratic politics, both of which (as Heclo and Wildavsky have shown) should take us directly into the corridors of power. Gray and Jenkins prefer to rely for much of their case material on official reports, as when, in a controlled way, the government allowed the Franks Committee to lift a few veils from the operation of the Falklands war. The alternative use of sources such as press leaks, ministerial memoirs, and interviews with principals, while they carry undoubted methodological costs, would have brought the authors closer to the administrative decisions that they attempt to put into new perspective.

Taken as a whole, the world that Gray and Jenkins depict is a highly political one. It is not that of the clever cynicism of the *Yes, Minister* television series, or of those who see these relationships primarily in clientelist or corporatist terms. Rather, it is a world of shifting power and dependency, in which the central, stable fact is that of organizations acting in reference to other organizations. A paraphrase of Michels might do to sum up their position: Who says organization says bureaucratic politics.

JAMES B. CHRISTOPH

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The Politics of Local Socialism. By John Gyford. (London: George Allen & Unwin, 1985. Pp. x + 129. $22.50, cloth; $8.50, paper.)

Traditionally, Britain's Labour party has understood local government as a means to implement or obstruct a parliamentary and centrally planned socialism and to provide employment programs and social welfare services. However, town center redevelopment and housing policies and garden city development revealed a party whose administrative and paternalistic politics were remote from constituents and occasionally corrupt. These programs often destroyed pieces of the urban fabric, disadvantaged the less affluent, and generated community opposition. As a result of electoral defeats, changing social and ideological composition, new leadership styles, and reorganized and increasingly problematic center-local relations, Labour has pursued a more decentralist form of socialist politics. This volume examines the democratic socialist potential of the political ideas and practices of newly emerging radical Labour councils in Britain's cities.

Gyford suggests multiple and interrelated sources of leftist Labour strength in urban government and electoral politics: Community Development projects; struggles and movements that develop around community action, women's and environmental issues, and economic and anti-austerity organizations; and a growing number of leftist party activists. Local socialism also has ideological roots: a refusal to accept a socialism that is bureaucratic and centralized, and a conviction that the local state is sufficiently autonomous and accessible to popular pressure.

He contends that the new urban Left's program is both defensive and prefiguratively socialist in nature. On one hand, it defends local autonomy against the Thatcher government and opposes its rate-capping legislation and attacks on welfare-state benefits and living standards. On the other hand, it pursues grassroots industrial policies planning to promote more unionized jobs and new, democratic forms of enterprise, employment, technology, and industrial control. The councils formally monitor police and seek to eliminate police racism and sexism. Also, women's social policies and services are high priorities. As resources for political education and community action, the councils' grant programs support local organizations in the arts, recreation, employment and training, women's services, and community development.

Proponents believe that the local road to socialism is distinguished from other current approaches to decentralization in at least two ways. First, local socialism links the people to the Labour party, opposes the policies of the Thatcher government, and defends working-class communities. Second, it encourages mass participation in the decentralized management of services and popular planning to improve their quality and match social needs with resources. Thus, the local state enables forms of urban political education, and the local state's power of example and of incubating innovative progressive programs educates the nation.

The councils face important and not unrelated political problems. They seek a new policy role for their members and more informal, less staff-dominated and professionalized forms of urban governance. They confront the strategic question of uniting old working-class constituencies with new and diverse cultural and ethnic group supporters. Although they combine an orientation toward constituency benefits with an ideological commitment to radical social change, unions and the Left frequently pressure them. In short, local socialism generates political conflicts over the functional decentralization, organization, and implementation of public services and programs, as well as over its forms of participation. It functions within significant, and in this work probably underemphasized, limits on local power and resources. If it escapes the dangers of party bureaucratization, it may still have slight impact on broader social and power relations. Finally, the strategic dilemma of local socialism is how to balance the desire for decentralization and self-determination and the need for political mobilization to establish new collective norms and transform the society.

Although some hold that local socialism rests on a consumption-based and irretrievably pluralist and localist politics, incapable of constituting itself as a national force, Gyford concludes that it has already influenced local government and the agenda for socialism. It has demonstrated that cities—governed with much less reliance on bureaucratic and professionalized norms and more responsive to diverse urban interests—can pursue and implement a more progressive range of politics and programs than many believe. Local socialism reintroduces the idea of a self-governing socialism based on the democratic practices, skills, and self-confidence of a participatory citizenry. Democratic socialist practice would encourage a healthy tension between parties and the people, and a variegated and decentralized economy. It would be willing to accept nonsocialist policies as the price of an unswerving commitment to a socialism that rests as much on political empowerment from below as it does on redistribution from above.

The book is intended as a mere introduction to local socialism in Britain. It succeeds only partially in that regard, for it does not clearly define and measure the likely or real, symbolic or tangible, policy accomplishments of local socialism. The broader comparative issues and profound theoretical questions are beyond its scope.

CHARLES E. ELLISON

University of Cincinnati

The Power of the Powerless: Citizens Against the State in Central-Eastern Europe. By Vaclav Havel, et al. Edited by John Keane. Introduction by Steven Lukes. (Armonk, NY: M.E. Sharp, Inc., 1985. Pp. 228. $35.00. Published simultaneously as vol. 15, no. 3-4 of *International Journal of Politics.*)

The book is the outcome of a joint Czechoslovak-Polish seminar on the nature of the Communist systems of East-Central Europe, and on strategies to promote civil and human rights in both countries. The leading essay was written by Vaclav Havel, a well-known Czech playwright, author, and member of the opposition. The volume consists of this lengthy piece and what may be considered short responses and elaborations by his Czechoslovak colleagues. The writers range ideologically from Petr Uhl, the founder of the Czech Revolutionary Socialist Party and the only one among the contributors in favor of violent socialist revolution in present day Czechoslovakia, to Joseph Zverina, a Catholic priest and theologian who advocates change through Christian love. All of the contributors are signatories of Charter 77, a document published that year in Czechoslovakia demanding the government respect human rights as guaranteed by its own constitution and by the international treaties to which Czechoslovakia is a signatory.

The essays provide an original and penetrating analysis of totalitarianism, or what Havel calls *post-totalitarianism,* which is bound to revitalize the ongoing debate among Western political scientists as to the usefulness of this term and the importance of ideology in these political systems. This view of totalitarianism is combined with a fascinating theory of how it could be effectively opposed. The originality of the discussion stems from the fact that it is undertaken, not by Western "specialists," who, in most cases, have a limited practical knowledge of these regimes, but by "practitioners"—either those who, like Hiri Hajek, were for many years members of the Communist ruling elite before they joined the opposition, or those like Reverend Zveryna, who spent nearly one-third of his life in Nazi or Communist prisons. Both groups know the system from the inside, but write with the considerable detachment so necessary for objective analysis.

The essays view the present-day totalitarianism in Czechoslovakia—and by extrapolation, in other countries of the Soviet bloc—as still ideologically based. This ideology however, instead of preaching the original message of total transformation, now serves as the gospel for the ritual fundamental to the maintenance of the system. The ideology, by its promise of the just society of equals, legitimizes the frozen system of inertia. The rulers and the ruled act as if the society were still faithful to its Marxist commitment to humanism and to the "withering away of the state." Officially only the myth is real, and one lives in a schizophrenic world where the real "real" is unreal. The total mobilization of society in early totalitarianism changed to a demand for total passivity and silence. In this system of "ifs" the Soviet Union cannot reform itself without destroying the prop on which the East-Central European "ifs" are built. One is reminded of Christianity, in which the original teachings of Jesus were eventually ritualized into power and corruption in the medieval church. Like the medieval church, Communist totalitarianism can only be destroyed or reformed by the truth about its lie. It has to be overthrown from within, in the tradition of Jan Hus, a Czech religious reformer who, one hundred years before Luther, preached the truth about the medieval church (and was burned at the stake). Today's totalitarianism can be defeated only if people live the truth in a parallel society, independent of government control—a society consisting of culture, media of communication, trade unions, foreign contacts, and even the economy—"a parallel polis." Living in the truth returns the focus from the state to the individual. The few who take the first steps and dare to live the truth may, like the early Christians, be thrown to the lions, but their example will create a multitude that eventually will conquer the empire.

Although the writers challenge the "real socialism" of present-day Czechoslovakia, they remain true to socialist values and, in most cases, to Marx and even to Lenin, because for them, socialism is the only system capable of extending democracy to economics. In their arguments there is a strong sense of the anti-statism of Marx and of Ghandi, in which the state is always viewed as a negative force counterposed against a "positive" society. Thus, the present socialist (Communist) state must be replaced by a loose association of self-governing groups with a minimum bureaucracy. The ideal model might be the Paris Commune of 1871, admired so much by Marx.

For an edited work the book is surprisingly consistent. It is definitely recommended reading for those with an interest in comparative politics, totalitarianism, political philosophy, and Marxism. These words were written by brave, intelligent, and dedicated men. It would be a pity if they were not heard in the West.

JAROSLAW PIEKALKIEWICZ

University of Kansas

Japan's Financial Markets: Conflict and Consensus in Policymaking. By James Horne. (Sydney, Australia: George Allen & Unwin, 1985. Pp. 271. $33.95, cloth; $13.95, paper.)

The recent gradual liberalization of Japan's financial markets has provided James Horne, an Australian government official, with material for a series of informative case studies about change processes in an important sector of regulatory policy. Following a brief but interesting overview of the government's relationship to financial institutions, he describes how a market for government bonds developed; the issue of whether banks or securities houses should control it; why certificates of deposit were finally authorized; conflict over interest rates and tax reporting in the postal savings system; the partial loosening of foreign exchange controls; and the growth of the market for foreign yen-bonds. The period covered is the 1970s and the early 1980s, with the most detailed information pertaining to 1980–81, when the author carried out over 100 interviews. There is also a good chapter on how policy might be affected by the Ministry of Finance's personnel system.

The key to Japanese financial policy is the maintenance of a highly regulated and hierarchical structure of low interest rates. Most of these stories are about how various actors try to preserve their own slice of the status quo as this structure is progressively threatened by the growth of government debt and the continuing process of internationalization. Regulated interest rates have some economic justifications, but the author's aim is to isolate political factors. In particular, the inertia he finds in this policy area is ascribed not to a Japanese love of harmony, but to intense conflicts of interest among multiple actors with effective veto power. This explanation leads to something of a contradiction, since both conflict and policy change are seen as increasing over time, even though conflict is supposed to inhibit change.

In general, theorizing is not a strong point in this book. The typology of decision making, crossing "level of agreement" and "level of concentration of control" to produce unity, consensus, dominance, and conflict modes, is largely circular and not very helpful. On the other hand, Horne is a clear-sighted observer of processes that have confused many political scientists. On the ever-popular question of whether the Liberal Democratic Party (LDP) or the bureaucracy really runs Japan, he concludes that even the Finance Ministry is "relatively hopeless in the face of effective political alliances built within the LDP"; yet in most of the cases described—some quite important—"the LDP had little or no direct involvement" (pp. 112–13). In fact, the majority party normally does not intervene, except when its grassroots political interests are at stake.

For the majority of policy issues, outcomes depend on the delicate relationships among bureaucratic agencies and between each agency and its clientele, and on the rules of the game (jurisdictions, routines) that pertain to particular decisions. Even within a circumscribed policy area like financial markets, decision-making processes get very complicated, and the author explains them well—he is especially good on the divergent interests of the four Finance Ministry bureaus active in this area. These are not always as high-minded as advertised: such motives as avoiding complications in the marketing of government bonds, protecting the profitability of banks, and even maintaining a pool of post-retirement jobs often outweigh economic policy considerations or pressures for change from foreign banks and governments. Horne thus takes a prominent place among the school of Tokyo watchers that tends to cast doubt on the Japanese government's capacity to make rational and coherent policy. Those who take the opposite view should be encouraged to consider his carefully assembled evidence from a crucial area of Japanese public policy.

JOHN CREIGHTON CAMPBELL

University of Michigan

The Geography of English Politics: The 1983 General Election. By R. J. Johnston. (Dover, NH: Croom Helm, 1985. Pp. xvii + 358. $43.00.)

With the zeal of a missionary among particularly recalcitrant natives, R. J. Johnston wants to alert political scientists to the importance of a "geographical dimension" in British political behavior. More particularly, he wants to banish forever the widely-held image of England as "a homogeneous place, with no spatial variations in voting behaviour additional to those that can be accounted for . . . by the basic electoral cleavages" (p. 278). The principal cleavage to which he refers is, of course, the traditional socioeconomic division of the vote with strong Labour support among working-class electors and equivalent or higher Conservative sympathy in the middle class. In making the case for localistic behavior during the 1983 general election, the author attempts to demonstrate wide variability in patterns of class-based voting among parliamentary constituencies, and to provide and verify sound theoretical explanations for local deviations from the nation-wide norm.

The homogeneity thesis is disproven quickly by showing how badly "national" patterns of electoral cleavage were reproduced across England's 523 parliamentary constituencies. When survey-based estimates of class voting patterns are applied to data on the social composition of the constituency, the simulated results diverge substantially from the actual election results in 1983. The author reports similar disparities when the individual-level voter transition matrix of 1979–1983 is used to forecast the recorded constituency-level flow of the vote. In a short but important chapter focusing upon the political consequences of voter behavior, Johnston demonstrates that constituency variations from the national pattern have important effects upon the distribution of seats in parliament.

For Johnston, the gap between forecast and outcome means that voters are influenced by constituency-level forces, which he apportions to *sectional, environmental,* and *campaign* effects. The first category, sectional effects, refers to local, regional, and other areal differences in partisan socialization and tradition. Environmental influences include the salience of particular issues and the personal qualities of candidates. The third type of local influence, campaign effects, refers to the closeness of previous campaigns and the scale of organized efforts to sway electors. Johnston argues that these forces are better explanations for spatial variations in class/party support patterns than the social interaction and "contagion" processes usually cited in studies of "contextual," "structural," "compositional," or "neighborhood" effects on voting.

The remainder of the book uses multiple

regression equations to account for spatial variation in several aspects of voting behavior: class-based voting, voter shifts in party preference since 1979, and class-based patterns of voter transitions. Lacking any direct information about such individual behavior within constituencies, and rejecting traditional methods for deriving cell entries from known marginals, Johnston generates the behavioral estimates for each constituency by maximum likelihood methods. Through this innovative technique, he discovers that the two major parties did better in winning and holding onto support from their "core" social class as that class increased its share of the constituency electorate. Moreover, Labour and Conservative candidates did better and held past supporters more efficiently among all classes of electors in areas where they ran incumbents for reelection or where regional traditions historically favored their party; in districts with clusters of certain occupations or characteristic population densities; and in marginal constituencies and constituencies where they spent more money. In addition, Labour gained support among all classes from high unemployment rates. Depending upon the particular dependent variable, support for the Alliance—the coalition between Liberals and Social Democrats—was less clearly predicted by any constituency characteristic.

The validity of the specific conclusions and of the author's brief for geographical influences hinges on the adequacy of the empirical analysis. However sympathetic the reader toward ecological analysis and contextual models, there are some troubling methodological questions left unanswered. The spatial variations Johnston attributes to various contextual forces might reflect measurement error or the differential distribution of certain unmeasured constituency traits. Because of sampling error, the national survey estimates of party support by social groups are approximations. The size of the social groups in each district's electorate is based on somewhat dated census information about the unit's resident population. If multiplying the two percentages produces a poor fit to recorded results, that might be due to the roughness of the estimates. Moreover, if individual voting is significantly affected by factors other than social group membership—as an increasing number of scholars now argue—then spatial variations in class/party relationships may be responses to variables not included in the model. The possibilities of measurement error and model misspecification should have been addressed. Given the book's five lengthy methodological appendices, I was surprised that the author did not raise these issues.

The pervasive indictment of previous voting research also seems overstated. Social scientists generally have not been as indifferent or naive about contextual influences as Johnston imagines. Recent political science literature provides a variety of contextual models that rely on behavioral mechanisms other than social contagion. Research on British electoral politics has suggested two major factors that might produce significant spatial variations beyond class—nonwhite immigration and "peripheral" nationalism. Racial effects are excluded from the analysis for no convincing reason (see p. 80), while the potential influence of peripheral nationalism is arbitrarily minimized by omission of non-English constituencies. Furthermore, Johnston's major empirical finding, that a party's support in its core class increases with the relative size of that class, is perfectly compatible with the contagion process that the author so readily decries. Until he can find better indicators for his alternative contextual influences and show their statistical impact, the social interaction model remains a plausible explanation for varying class/party relationships across election districts.

Given its theoretical and methodological complexity, the book is unavoidably heavy going. Despite dense prose and occasional howlers like the use of "preciser" (p. 107) and reference to data that was "grossed up" to another level (p. 110), the argument is comprehensible. More important, for all the weaknesses already enumerated, the argument is stimulating. Like other contextual research, the book raises doubts about the adequacy of the individualist model of voter behavior that still pervades much research in Britain and elsewhere. Thus, the book can be read with profit by both students of British politics and experts in the field of voting studies. Even if the magnitude and source of geographical influence on voting remains a matter of debate, its existence warrants the type of sophisticated analysis exemplified in this book.

KENNETH D. WALD

University of Florida

State-Building Failure in British Ireland and French Algeria. By Ian Lustick. (Berkeley: Institute of International Studies, University of California-Berkeley, 1985. Pp. x + 109. $8.95, paper.)

Lustick argues that the presence of British and French settlers was "a sufficient, albeit not necessary" condition for the unsuccessful incorporation of the "subject peripheries" of British Ireland and French Algeria into the British and French states (p. 5). The bulk of his evidence for this claim comes from secondary source material on the consequences of "settler political activity for cooptive and participatory native policies pursued by metropolitan cores" (p. 9) for Britain (pp. 17–38) and France (pp. 47–76). Lustick also uses material about the successful cooptation of local elites and hence the successful incorporation of subject peripheries by Britain and France in their earlier history (pp. 9–16). Lustick's explanation of state-building failures in Britain and France is an important and clever addition to the literature on colonialism. He argues that the failure results from the relationship between settlers, natives, and the metropole.

To expand the central state, the metropoles had to overcome the demographic preponderance of the natives. Their solution was to settle large numbers of nonmilitary citizens in outlying territories inhabited by "culturally distinct and antagonistic natives" (p. viii). The British thus planted colonies in Ireland in the 1580s (Elizabeth), from 1607 to 1640 (the Stuarts), and in 1652 (Cromwell); France planted colonies in Algeria from 1830 to the early 1900s. However, in planting these colonies, the metropoles pursued contradictory goals. During tough times, the colonies were viewed as an asset to be exploited—needing the natives in order to consolidate their rule, the metropole accommodated native demands by attempting reforms. During good times, the colonies were viewed as a burden to be ignored—not needing the natives, the metropole accommodated its settler's demands and ignored reforms.

Settlers also pursued contradictory goals. On the one hand, they were numerically weak compared with the natives, and hence wanted protection against potential native mobilization. The settlers thus sought close ties and assimilation to the metropole, so that they could use its resources to maintain local hegemony. On the other hand, the settlers wanted to dominate the native majority and protect their local privileges. They thus sought autonomy from the metropole, so that they could escape the logic that full integration into the metropole meant equal treatment of all citizens, settlers and natives alike.

Through various mechanisms (pp. 81–82) the settlers were able to undermine the metropole's cooperative policies towards the natives, and hence block reforms. The consequences were that indigenous local elites were not coopted into the decision-making process, and native masses did not enjoy full citizenship rights. These factors led to native unrest, a struggle for political rights, separatist demands, and the prevention of the transfer of loyalty and legitimacy from the core to the periphery.

Lustick's argument is intriguing, but based on two crucial assumptions. He assumes, first of all, that in the absence of pressures from settlers, Algerian and Irish ethnic groups would be reformist, and that native elites would welcome metropole reform policies (pp. 37, 70, 72). In other words, Lustick assumes that settlers disrupt "processes of elite cooptation and expansion of political participation rights to natives" (p. 8) that are sufficient for successful integration. However, would the native elites ever have been reformist and coopted? The argument tends to slight other factors (e.g., geography, culture; see p. 83) that influence separatist and radical ideologies and behaviors among ethnic groups.

Second, the argument assumes that metropole policies would be reformist in the absence of settler pressures, but how generous are metropole elites? Are they ever willingly reformist and accommodative of native demands? The argument tends to ignore other factors (e.g., interelite sqbbles) that influence metropole policies. Lustick assumes that accommodative metropole policies are sufficient for successful integration.

To test Lustick's argument, one thus needs a crucial case study: if settler undermining of metropole policies is decisive in preventing successful incorporation of peripheries, then a situation where cooperative natives rebelled against reformist metropoles stymied by hegemonic settlers should be analyzed. Lustick did not establish that, *ab initio*, the natives were cooperative and the metropoles reformist. This volume thus establishes only the plausibility of Lustick's argument. It is an interesting and important attempt to "describe, compare and explain the differential success of various state-building efforts" in Britain and France (p. 2), but a convincing test of Lustick's argument has yet to be made.

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Capitalism and Apartheid: South Africa, 1910–1984. By Merle Lipton. (Totowa, NJ: Rowman and Allanheld Publishers, 1985. Pp. xii + 449. $19.95.)

The historical and contemporary relationship between capitalism and apartheid is an issue of significant debate among scholars of South African political economy. To cast it starkly, some maintain that apartheid structures, which place severe limitations on the vertical and horizontal mobility of African labor, were the creation of irrational and backward forces of Afrikaner nationalism that obscured capitalist development. Capitalists, therefore, have never really wanted apartheid, and can be expected to press for its removal. Others argue that capitalist forces helped to construct apartheid and benefited enormously from the cheap labor it guaranteed. Thus, the racist structures were not an irrational impediment to capitalist growth and profit, nor can capitalists be expected to be a progressive force to help eliminate apartheid. Lipton engages this debate in a complex and dynamic study that demonstrates the crudity and static nature of some previous formulations.

The heart of Lipton's analysis is contained in the book's middle section, which systematically examines the changing interests of the "white oligarchy." Included in the oligarchy are agricultural, mining, and manufacturing capitals, and white labor. The author asserts that the exclusion of Africans from political and economic power was rationally calculated and vital to white (predominantly Afrikaner) farmers' interests because

> it underwrote their possession of the land, shielded them from black competition and provided them with plenty of cheap docile labour, while not imposing on them the costs in the skilled [white] labour market incurred by other employers. (p. 108)

White labor was the other major economic sector that demanded apartheid in order to protect itself from the higher quantity and lower costs of African labor.

Mining capital, dominated by the English, had a more complex calculation of the costs and benefits of apartheid. In the beginning, white conquest was essential to the operation of the mines, and later mineowners supported the restrictions on Africans' physical mobility. However, these capitalists did not want or like the job bar that inflated the price of white labor by assigning all whites to skilled jobs and all Africans to unskilled jobs. Forced to accept this piece of apartheid, mineowners responded by instituting highly repressive measures against Africans, such as closed housing compounds and very low wages. In addition, the increasingly interventionist role of the state was resented by owners of mining companies, whose surpluses were taxed to pay for the protection of agriculture and manufacturing. Manufacturers generally opposed apartheid because it was incompatible with their need for skilled black labor and a larger domestic market. This sector was compensated for such costs through protectionist policies that, together with repressive measures by the state, helped assure its acquiescence.

In the 1970s, Lipton asserts, the benefits of apartheid to certain sectors of the white oligarchy that had previously supported it began to change. This change was brought about by increasing capital intensity in all sectors of the economy that required even more stable, skilled labor—the kind that apartheid was designed to discourage among Africans. Support for apartheid among capitalists of all kinds has dramatically declined in the last 15 years, Lipton argues, although it has remained high among white labor and the state bureaucracy. The resulting conflict within the oligarchy has produced "a contradictory and confusing mixture of reform (particularly in the socio-economic sphere) and maintenance of apartheid (especially politically)" (p. 49). Capitalists have not forced more changes because of divisions among them, their inability to forge coalitions with the black opposition, their fear that instability and insecurity would accompany rapid change, and state repression.

In addition to providing an engaging, readable, and lucid analysis of the differing economic and political interests of the ruling elite, Lipton provides an overall history of twentieth-century South African political economy, an analysis of the impact of apartheid on blacks, an examination of the interaction of race and class as variables affecting policy and social change, and an investigation of historical and recent changes in apartheid.

An issue taking on increasing significance, but which is inadequately addressed, is the potential for effective black resistance. Lipton discusses this, but does not devote to it the careful analysis she brings to the examination of South Africa's rulers. Perhaps further elaboration on this issue is too much to ask of one book that is already broad in scope and length. The occasional use of survey data collected by others in a virtual police state, especially to analyze black opinion, is a questionable practice. Another disappointment is the lack of a comprehensive bibliography, although there are extensive footnotes. These quibbles aside, the book is excellent scholarship and a refreshingly sophisticated contribution to the study of South African political economy.

JANICE LOVE

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Economic Policy and Policy-Making under the Mitterrand Presidency, 1981–1984. Edited by Howard Machin and Vincent Wright. (New York: St. Martin's Press, 1985. Pp. x + 293. $27.50.)

The surprise of the Socialist electoral victories of 1981 inspired much prudence among students of French politics. Only recently has confidence returned to the field in a quantity sufficient to allow observers to commit their appreciations of French politics in the eighties to print. Hence, one welcomes this edited volume on Socialist economic policy making. The welcome is all the warmer because of the book's focus. Economic policy was the one facet of the Socialist victory that aroused the greatest hopes and the greatest curiosity, and it is Socialist economic policy that has provoked the greatest disappointment.

The editors bring to this enterprise a collection of eminent scholars. Primarily an Anglo-French endeavor, its authors include such well-known and well-placed French observers as Paul Fabra of *Le Monde,* Dominique Strauss-Kahn of the *Commissariat au Plan,* Pierre-Alan Muet of the *Observatoire Français des Conjonctures Economiques,* Christian Stoffaes of the Ministry of Industry and Research, as well as academics such as Yves Mény and Yves Morvan. Three articles were authored by respected American observers of French politics: Janice McCormick, Suzanne Berger, and Frank L. Wilson. The British contribution, with the exception of the introductory article by Machin and Wright and a provocative article on the *lois Auroux* by Duncan Gallie, is confined to commentary on the articles. The commentary is both informed and perceptive, however, and provides much of the appeal of the book.

One finds in this volume a diversity of appreciations concerning Socialist economic policy. For some, Socialism represents little more than an abortive experiment in idealism. At best, the Socialist reign has "performed an educational function" by allowing the left, excluded from power for nearly a generation, to learn "how to manage France in an interdependent world economy" (McCormick, pp. 60–61). One encounters ironic continuities between the policies of the current government and those of its predecessor. Jack Hayward points out that "despite the scorn poured on the still-born Eighth Plan, its Socialist successor was often forced to follow it closely" (p. 114). One encounters similar continuities in financial policy, despite the Left's criticisms of the financial strategy pursued by the Giscard-Barre government.

Instances abound in which the best-intentioned policies yielded unanticipated outcomes. For instance, the government sought to restore the Plan to a position of preeminence. However, it actually "accelerated the decline of planning" (Ozenda and Strauss-Kahn, p. 112) by promoting greater *concertation* (cooperation) between the state and interested social groups, thus depriving the planning commission of autonomy. In the same vein, the government's privileged relations with the leftist unions resulted in a loss of influence for the latter, leaving the government with ineffective union support. Similar disappointments have sullied the Socialist government's attempts to diminish the influence of the *patronat* (employers), as well as its attempts to reinforce worker support through reforms in industrial relations.

On the other hand, some contributors take care to identify the benefits—actual or potential—that have accrued as a result of France's experiment with "socialism." Christian Stoffaes, for example, points to the reforms that have taken place in the management structures of nationalized firms, and predicts that if such reforms succeed and spread to the private sector, then "the coming to power of the left will have created a new situation in industrial relations in general" (p. 158). Nevertheless, one closes the book pondering the words of David Goldley, for whom "it would not be the least paradox of the Socialist experience in France, if the Government were rejected for its excessive and broken promises of 1981, but in fact left to its conservative successors a much better situation than it itself inherited" (p. 254).

This volume is not characterized by an ambition for anything like theoretical sophistication. It is a book of political analysis, and the level that it achieves within its chosen genre is very high indeed. Nevertheless, its pages contain a number of intriguing insights of theoretical relevance. For example, both Machin and Wright's and Janice McCormick's articles refer to the limits of the state's power to formulate and implement economic policy. Jean-Paul Fitoussi writes in his comment that "today, State power is relatively much weaker for, domestically, it is trapped in corporatist arrangements and externally, it is even more constrained by the rules of international competition" (p. 166). The state theorist will also find provocative the claim made by David Soskice that "the case for state intervention in French industry rests partially on the strength of the State, but also on the inability of French industry to restructure itself, through lack of coordination" (p. 171).

In sum, this is a book replete with both solid analysis and provocative insight. It is certain to reward the efforts not only of the student of French politics or of economic policy, but also of the student of state theory and international political economy.

MICHAEL LORIAUX

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Marxism in Asia. Edited by Colin MacKerras and Nick Knight. (New York: St. Martin's Press, 1985. Pp. 297. $32.50.)

The stereotype of monolithic Marxism so tenaciously popular in the United States could not survive a reading of this book. Not that all the Marxisms described are attractive or even harmless: it is numbing to read even a short description of North Korea's *Juche* ideology, and the Khmer Rouge slogan of "Spare them, no profit; remove them, no loss" is as brutally simple as their killing fields. However, the ideologies have so little in common with each other and are so tightly interwoven with the political and cultural context of each country that their common allegiance to "Marxism" is more historical than theoretical.

Marxism in Asia is intended to be an advanced undergraduate textbook. Except for its price it fulfills that purpose well, although "Asia" should be understood as East and Southeast Asia. Within that restriction the important Marxisms of the Philippines, Malaysia, Burma, Thailand, and Laos are left out. The book concentrates on the ideology of the respective communist parties. It is not concerned with salon Marxism, and it presents the political history of the parties only insofar as is necessary to understand ideological developments. Generally speaking, the introductory material on Marxism, Leninism, and Stalinism, the extensive treatment of China, and the individual chapters on North Korea, Japan, Vietnam, Kampuchea, and Indonesia provide a solid introduction to the respective ideologies that is as clear as the subjects permit. Some parts of the book are especially good—I would single out the introduction to Marxism-Leninism, the discussion of Mao Zedong, and the chapters on Japan and Indonesia. The weakest chapters are those dealing with post-Mao China and North Korea, but even these are worth reading. Anyone desiring a beginning orientation to Asian Marxisms would find the book useful.

The introductory material on Marx, Lenin, Stalin, and the Comintern provides a clear introduction to the common heritage of Asian Marxisms. The discussion of Chinese Marxism develops into an analysis of Mao Zedong that makes understandable both the connection of Mao's politics with his more abstruse theorizing and also the changes between his Cultural

Revolution leftism and his earlier thinking. The succeeding chapter tends to treat post-Mao Marxism as the denouement of Maoism, which is unfortunate because there have been many interesting and challenging ideological developments. It is unfair, for instance, to say that the Chinese have given up "systematically seeking social equality" and that "they have devised a theory based on Marx to justify present inequality" (p. 136). Equality has not simply disappeared as a political value; it has become a problematic and sensitive one. Rawlsian justifications of dynamic inequality are heard countering murmurings about polarization, and Gini indexes, caloric intake, and other welfare indicators have replaced class struggle as the ammunition of argument. It is a hard task to summarize and simplify post-Mao developments, but there are much better attempts by Schram and Tsou.

The second half of the book is comprised of chapters of from 20 to 30 pages dealing with each of the remaining countries. The problem with the Korean chapter is that Kim Ilsungism is taken too much at face value, and there is not enough analysis of its pathology. It is a wonder to read that "*juche* and Kim Il Sung's thought are indeed a creative development of Marxism-Leninism" (p. 171). Dynastic Stalinism is different, but I am not sure that it is creatively different. The chapters on Japan and Indonesia not only provide a summary of ideological developments, but succeed in conveying the unique spirit of Marxism in these countries. Even though these are the two "parliamentary Marxisms" of the collection, the highly factional and intellectual soul-searching of the Japanese contrasts dramatically with the broad themes of national unity in Indonesia. A very different and chillingly unique spirit of the Khmer Rouge is presented in a chapter entitled "Kampuchea and Stalinism." The portrayal of the Khmer Rouge is useful and convincing, but the comparisons with Stalin and Mao are haphazard. It would have been useful to extend the analysis to the current government of Kampuchea, because it presents a peculiar case of one Marxist government overthrowing another. The treatment of Vietnam is adequate, but the very important reforms since 1980 are ignored. The overall development of politics in Vietnam is more complex and interesting than the chapter suggests, but part of the reason is that the Vietnamese are cautious in theorizing. Perhaps for good reason. The leading theorist, Truong Chinh, holds some personal responsibility for two major policy disasters.

So what is Marxism in Asia? One could easily collect a stew of contradictory adjectives from this book, and if more countries were studied the pot would more easily boil over. The greatest common denominator would be pitifully small, and moreover, would fit a number of non-Marxist political movements. The ideological connection has become more genetic than generic. *Marxism in Asia* succeeds with its subject only because it starts from the individual context of each country, rather than from a Procrustean bed of stereotypes or formulas.

BRANTLY WOMACK

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State and Opposition in Military Brazil. By Maria Helena Moreira Alves. (Austin: University of Texas Press, 1985. Pp. xiv + 352. $22.95.)

Brazil has long been considered a paradigmatic case in analyses of bureaucratic authoritarianism in Latin America. In 1985 Brazil witnessed the installation of a civilian in the Planalto presidential palace after a long period of military rule initiated by a coup in 1964. Consequently, Brazil, the eighth most powerful economy in the capitalist world and a leader among the "advanced developing countries," currently figures prominently in the study of democratization in the region. The publication of *State and Opposition in Military Brazil*, therefore, should be welcomed by "Brazilianists" as well as by those interested in the comparative analysis of authoritarian rule and regime transitions.

Moreira Alves's ambitious objective is to examine the "dialectical relationship" between the state and the opposition, in order to understand the forces that have "shaped the complex political and social mechanisms of domination in that country since 1964" (p. 3). This research is informed by skillful use of documentation drawn from governmental sources and the major opposition actors in civil society. Judicious use is also made of personal memoirs and the author's more than 100 interviews with opposition figures. Useful statistical data is presented in a 40-page appendix. Very little has escaped Moreira Alves's encyclopedic coverage, although some topics, such as the "culture of fear" and the key role played by entrepreneurial elites, could have been accorded more extensive treatment.

The central focus is on the "Doctrine of National Security" and its offspring, the "national security state." Four contradictions in the Brazilian variant of the national security state are identified: (1) a tendency toward unchecked expansion of the state's repressive apparatus; (2) the state's inability to eradicate opposition completely; (3) state repression that tends, in the context of mounting social and economic grievances, to propagate opposition from one sector of civil society to another; and (4) the abyss between the language of legitimation premised on the return to democracy and the reality of state repression, which tended to generate recurrent politico-institutional crises.

Following this initial discussion, Moreira Alves turns to a careful narrative reconstruction of three "stages of institutionalization" of the post-1964 military regime. Her goal is to demonstrate that the process of "decompression" and *abertura* (political opening) under military rule stemmed from constant transformations in the state's relationship with a multifaceted opposition movement rooted in civil society. Herein lies the book's most significant departure from the English-language literature on contemporary Brazil. Moreira Alves parts company with the view that liberalization was induced by the post-1974 collapse of Brazil's "Economic Miracle," as well as with those who argue that liberalization was the consequence of the ascendance of more liberal generals with a moral commitment to return the country to democracy and the military to its barracks.

Eschewing both economic and psychological reductionism, Moreira Alves demonstrates that

> after each period of repression (which reflected the view of the "internal security" sector within the military) the state would implement a policy of gradual liberalization in order to lower the level of tension created by the violence of the repressive apparatus. (p. 255)

Analysis of the last two military governments headed by Generals Geisel (1974–1979) and Figueiredo (1979–1985) indicates that the dynamic of change cannot be accurately described as a linear progression from liberalization toward democracy. Each successive cycle of liberalization was a response to the growing capacity of opposition sectors to resist a logic of state power crystallized in the national security doctrine. Decompression was a deliberate strategy articulated by General Golbery do Cuoto e Silva, the *éminence grise* of the post-1964 period. First, the regime deployed a policy of carefully targeted selective repression, and second, it simultaneously pursued controlled political liberalization, circumscribed by greater respect for individual rights and limited electoral competition. This two-pronged strategy did not envisage a transfer of executive power to the opposition; rather it was intended to curtail what General Geisel once referred to as "useless violence" and to legitimate authoritarian rule.

While generally persuasive, there are some ambiguities in this thesis. Moreira Alves may overstate the regime's self-consciousness, foresight, and capacity to manipulate alternative political scenarios in a manifestation of sovereign will. In fact, the thrust of the study's insights and original research on elite and mass opposition appears, at least partially, to contradict the author's interpretation. Events subsequent to the book's completion, culminating in the return to civilian rule in March 1985, illustrate the accelerating disorientation of the regime and its loss of control over the tempo and final destination of the *abertura* process. Notwithstanding this quibble, Moreira Alves makes an important contribution to debates concerning the role of the opposition—particularly labor and grassroots movements—as Brazil under the *Nova República* navigates between another episode of elite conciliation from above and the effort to consolidate democratic institutions and foster a participatory political culture.

This commendably detailed study diverges in several respects from existing interpretations. Perhaps most significantly, it questions the current view that Brazil exemplifies a "transition from above," stage-managed by the authoritarian elites themselves. Moreira Alves does not explicitly address this issue, but the primordial role of the opposition in the Brazilian transition suggests a need for a rethinking of current typologies of regime change. Indeed, *State and Opposition in Military Brazil* is a compelling argument on behalf of the importance of well-done studies of specific historical cases as the only legitimate basis for valid comparative analysis.

William C. Smith

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Politics in Mexico. Edited by George Philip. (Dover, NH: Croom Helm Ltd., 1985. Pp. 223. $27.50.)

This edited collection of essays came from a two-day conference held in June 1984 at the Institute of Latin American Studies in London organized by the British-Mexican Society. George Philip, lecturer in Latin American politics at the London School of Economics and the London Institute of Latin American Studies, skillfully coordinated a panel at that conference that related Mexico's financial crisis, raging since 1982, to the political pressures especially evidenced in the fraud-tainted elections in 1982 of congressional and gubernatorial candidates of the dominant Institutional Revolutionary Party (PRI). (Though held a year after the conference, the controversial election of *priistas* (members of PRI) in the congressional and gubernatorial elections of July 1985 provides further evidence for the pessimism of some of its political analyses.)

In the opening chapter, "Causes of Mexico's Financial Crisis and the Lessons to be Learned," William Chislett of the *Financial Times* of London orients readers to factors found throughout the volume. He analyzes Mexico's huge foreign debt against the background of the Mexican oil syndrome. Norman Cox, British ambassador to Mexico during 1978–81, succinctly summarizes the Mexican political system, showing how the dominant PRI must deal with the Communist left and the traditional right. Editor George Philip furnishes a chapter, "Politics Under Stress," that shows clearly that the De la Madrid administration has tried to break sharply from its two immediate predecessors under Luis Echieverría and José Lopéz Portillo.

Professor Antonio Juarez of the National Autonomous University of Mexico (UNAM) describes Mexico's Marxist left and its background. Economist Barbara Wijngaard provides economic perspectives. John R. Heath, a research fellow at the Agrarian Investigation Center in Mexico City, traces Mexico's food policy, linking land tenure decisions to other government programs. Diane Stewart, a research student at St. Antony's College, Oxford, untangles the enigma of Mexico's banking system, complicated by the 1982 bank expropriations.

Roman Gío Argáez, research student at Essex University, covers Mexico's foreign policy during 1982–84, taking the Contadora activities more seriously than have any of the Central American nations more directly involved. Other papers on Mexico's stream of illegal migrants into the United States and on agrarian development complete the volume. In light of the several political and economic crises that have occurred one after the other since 1982, this volume is welcome among those trying to keep up to date on Mexican public life.

Reviewing this book in early 1986, right after the announcement that falling oil prices are squeezing Mexico almost to default, this reviewer, living in the Mexican borderlands, has had to try to resist the escalating pessimism about Mexico. However, the once-bright hopes for the De la Madrid term (1982–88) have dimmed. The Mexican government—risking political fallout—has boldly pursued austerity measures, many of them in 1985 after this book went to press. For example, federal spending was cut 15%, and hundreds of political appointees were dropped from token jobs not guaranteed by the civil servant unions.

Some of the De la Madrid innovations are working, so veteran Mexicanists cannot really proclaim Montezuma's malaise. This splendid little British volume will help political scientists reach an evaluation of Mexico somewhere between the bravado of the government's daily newspaper *El Nacional* and the nattering negativism of the *Washington Post*'s correspondent in Mexico City. Technocratic leadership can easily be discerned by those attuned to the Mexican mystique. That leadership stubs its toes now and then but does manage to keep governmental and private-sector workers functioning—probably better than counterparts elsewhere in Central America and the Caribbean.

Marvin Alisky

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The Evolution of Modern Botswana. Edited by Louis A. Picard. (London: Rex Collings, and Lincoln: University of Nebraska Press, 1985. Pp. 339. $37.95.)

The importance of Botswana in the study of African politics is all out of proportion to the size of its population and its historical impact on the development of the rest of the continent. Two factors provide the basis for elevating Botswana to the level of a "critical" case study: (1) its status as sub-Saharan Africa's only independent state with an uninterrupted tradition of democratic government; and (2) its strategic location in the heart of southern Africa. As expected, this interesting and useful collection of essays addresses both of these important issues.

This book, which has its origins in a panel at an African Studies Association convention, brings together 11 essays by 10 different authors, all of whom have done extensive fieldwork in the region. The book is divided into four sections, the first two dealing with the historical development of Botswana; the third section treats the political system, with emphasis on rural development and local administration, and the fourth examines the role played by Botswana in the international arena.

The major emphasis in the work is on the issue of local institutional development, a question addressed by 5 of the 11 essays. Especially interesting is the essay by Malcolm O'Dell, which reexamines the role of the village *kgotla,* a sort of combination local court and town meeting. He suggests that these vital institutions have been largely overlooked as the basis for rural development in the country. Robert Hitchcock, in his examination of land tenure (particularly that regarding pasture areas) emphasizes the complexity of the traditional tenure system and its relation to current government development policies in the livestock sector. Picard and Morgan provide interesting insights into the problems associated with policy implementation through local institutions. This is an important addition to the all-too-limited literature on problems of implementation at the local level in African polities. The remaining two essays in this section provide a summary of the functioning of the party system (Picard) and an assessment of the utility of social class theory as an explanation of rural development in Botswana (Holm).

The historical section is composed of a brief introduction to and summary of the historical development of Botswana (Picard) and essays by Q. N. Parsons and Jack Parson. Parsons offers some reinterpretation of several facets of the nation's history and their likely impact on future events. The third historical chapter is a somewhat unsuccessful attempt by Jack Parson to apply dependency theory to the history and development of Botswana.

The last section of the book moves us into the international arena, with good summaries of the problems associated with Botswana's external linkages. Dale's contribution provides a good description of the actors involved, while Polhemus focuses on the situation of the country in relation to the liberation struggle in South Africa. The final chapter provides an interesting, but not surprising, set of conclusions by Weisfelder, who apparently played the role of discussant for the original conference panel.

The main weaknesses of this volume are the lack of a consistent theoretical theme and the fact that there is little new empirical data presented in the various chapters. The book does, however, bring together good summaries of much past research done on Botswana, and should thus be regarded as a valuable addition to the libraries of those with an interest in the region in general, and Botswana in particular.

RICHARD VENGROFF

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Cities and Services: The Geography of Collective Consumption. Steven Pinch. (London: Routledge and Kegan Paul, 1985. Pp. xi + 213. $29.95.)

British urban studies have been flourishing vigorously during the last decade, chiefly because they stand at the crossing point between two streams of theoretical development. The first and older, established stream includes American-dominated liberal approaches to analyzing cities and urban problems, especially the contributions made by neoclassical economics, formal spatial modeling, descriptive sociology, and the community power debate and its aftermath. The second stream consists of the newer, French-influenced wave of Marxist and neo-Weberian sociologists, for whom questions about the production of cities have been far more important than their ideologically magnified social effects. The British literature has tried to meld together some behavioral research technologies with modern urban social theory. Nowhere has this empirical drive been stronger than in a new political geography seated firmly in a preoccupation with urban problems, and trying to connect some inherited geographical concerns about the spatial implications of public policy decisions with the substantive analysis of urban social conflicts and their political management. Steven Pinch's book provides the latest in a line of increasingly plausible attempts to make this amalgam work.

Cities and Services marries two liberal and two newer themes. The first is the impact of jurisdictional fragmentation among local authorities (and state governments in the U.S.) on citizens' access to services, which is handled both by conventional geographical mapping of accessibility to services and by comparative output studies of subnational governments' service provision. The second liberal theme deals with externality problems, the tapering of access to locational facilities with increasing distance, and the general problems of locational conflicts between incompatible land uses. The first of the newer themes concerns the institutional determinants of urban managers' behavior, analyzed in largely Weberian terms of the dynamics of bureaucracy. The last theme addresses the involvement of conflicts over urban public services in three contexts: (1) in broader social struggles, (2) between production and consumption imperatives, and (3) between capital and labor. The links between all these theoretical approaches and particular urban policy problems are convincingly made in a way that students especially will find very useful. These core chapters are topped by a short account of the structures of local government in Britain and the U.S., from which all the book's empirical examples are drawn. A concluding chapter focuses on a number of disparate theoretical developments that are supposed to take us "beyond locational analysis and structuralism." While each of these elements is fairly summarized, there is no really effective synthesis here.

Overall, Pinch has written a useful student text that should have a wide readership within courses tackling urban politics or policy analysis. His consistent concern with the spatial patterning of service delivery may put off some of the *Review*'s readers, but this is an important theme that connects very closely with the practical concerns of public administrators and policy analysts about territorial equity, and that political science has tended to neglect. Pinch addresses these issues from a variety of theoretical approaches, drawing on welfare economics, public choice theory, and sociology, as well as on political science and geography.

At a deeper level, *Cities and Services* has some flaws. Its wide attention span occasionally induces a feeling of ungrounded knowledge, as when a variant of substantive individualism is described as "methodological individualism" (p. 6), or when pluralism is classed as a subset of public choice theory (p. 34). The grouping of new developments into the last chapter gives an anachronistic feel to some preceding text, as if the author's reading had rather lapsed after 1981, or the publishers had tarried overlong in getting the book into print. The exclusion of the foreign language urban studies literature is also restrictive, given the bridging role of British urban studies mentioned above, and the adaptation to a period of public service cutbacks and deurbanization seems curiously patchy. Despite such limitations, however, Pinch is a lucid, reliable guide to a complex literature.

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Yugoslavia in the 1980s. Edited by Pedro Ramet. (Boulder, CO: Westview Press, 1985. Pp. xvi + 354. $27.00.)

Many outside observers felt that the death of Josip Broz Tito in May 1980 would unleash a host of cataclysmic events in Yugoslavia, bringing civil war, incorporation into the Soviet bloc, a military coup, or the rise of a new but less charismatic, legitimate, and capable leader. Happily for Yugoslavia, these visions turned out to be fantasies. The collective leadership designed by Tito to manage his succession proved durable and highly successful in avoiding spectacular catastrophes. Unhappily for Yugoslavia, Tito's success in institutionalizing this decentralized, almost confederal system has left a legacy of substantial problems: a stalemated government that cannot respond effectively to the nation's intensifying economic crisis, which in turn has produced growing social disillusionment. Pedro Ramet's *Yugoslavia in the 1980s* provides an excellent description and analysis of this growing economic-political-social crisis and paralysis. Unlike most such collections, the contributions are well chosen and integrated to provide a comprehensive picture of contemporary Yugoslavia.

Chris Martin and Laura D'Andrea Tyson describe Yugoslavia's stagflation of the early 1980s, and show how the economic and political institutions constructed in the 1970s perpetuate economic irrationalities and prevent effective policy responses. The problems of political organization are examined in more detail in several other chapters. Wolfgang Höpkin discusses the inconclusive debate over how to reform the League of Communists to reverse its fall "from federalization to disintegration." Dennison Rusinow describes the similar stalemate concerning Yugoslavia's manifest nationality conflicts, and notes how bureaucratic self-interest is changing some of the conventional positions of specific national or ideological groups. Robin Remington discusses several issues in civil-military relations, and concludes that the military is, at least at present, well integrated into the governmental system.

Several case studies indicate how federalization and decentralization have brought policy paralysis. Sharon Zukin analyzes the growing problems in and popular disillusionment with Yugoslavia's unique system of self-management in enterprise administration and local government, and Barabara Jancar demonstrates that the self-management system has inhibited attempts to combat environmental problems. The other chapters on domestic politics also indicate the regional variations and or increased questioning of the current system in the role and autonomy of the press (Pedro Ramet), religious policy (Pedro Ramet), and the generation gap and the growth of "feminism" (Barabara Jancar). In the realm of foreign policy, Zachary Irwin describes Yugoslavia's nonalignment policy, especially her competition with Cuba over the direction of the Nonaligned Movement, and Othmar Nikola Haberl argues that the central facets of Soviet-Yugoslav relations in the post-Tito period are "reciprocal pin-pricks" politically, and Yugoslavia's growing dependence on the USSR economically.

These essays are well integrated by two theoretical overviews. George Schöpflin presents a model of "political decay" in Communist systems, and shows how it applies to the somewhat different situation in Yugoslavia. (As Schöpflin and several of the other authors imply, a significant cause of Yugoslavia's decay has been the adoption of political reforms that remain anathema to orthodox Marxist regimes.) Pedro Ramet argues that Yugoslavia represents an "apocalypse culture" that is "prepared to question the fundamental political and social values of the society" (p. 3) because of obvious political and economic breakdown. These two theoretical perspectives raise broader questions relating to the study of comparative communism that go beyond the book's excellent conception of contemporary Yugoslavia: Will economic stagnation produce crises of political legitimacy in other Communist states (as has, in fact, occurred in comparatively "liberal" Poland)? How will more orthodox regimes respond to the growth of apocalypse culture? Will the current Yugoslav deterioration inhibit political reform in other Communist countries? There is also some grist for democratic theorists in this volume's unstated central theme that the Yugoslav regime instituted the self-management system in the belief that participation can be an "opium of the people," but found to its sorrow that bread is more important.

CAL CLARK

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Soviet Politics in the 1980s. Edited by Helmut Sonnenfeldt. (Boulder, CO: Westview Press, 1985. Pp. x + 247. $24.00, paper.)

Soviet Politics in the 1980s is remarkable because of the uniformly high caliber of all 16 chapters, each written by a different scholar. It covers the period from 1960 to 1984. More than half of the chapters deal with the economic life of the Soviet Union, and each chapter presents a rigorous analysis of Soviet economic stagnation and examines the plans and prospects for reform. There are a number of very informative tables and charts pertaining to the growth of the Soviet economy and to trade between the Soviet Union and the outside world. Each chapter has a very up-to-date bibliography to the end of 1983.

Five chapters deal with political and military matters. Of especially high quality are Thane Gustafson's "The Andropov Accession," Steven Rosefielde's "Soviet Arms Buildup or US Arms Decline?," Dimitri K. Simes's "Can the West Affect Soviet Thinking?," Wolfgang Leonhard's "Soviet Foreign Policy: Interests, Motives and Objectives," and Fritz W. Ermath's "The United States and the Soviet Strategic Challenge." The final two chapters are really a brief essay by Richard Pipes, "US Policy Opportunities," and a short speech by Lawrence Eagleburger, "US National Interests and the Soviet Union."

This book constitutes a periodic check up of the body politic of Soviet totalitarianism by a highly specialized team of experts at a time of internal erosion and external expansion in the USSR. It should be of particular interest to policy makers in Washington, for whom this work was primarily intended. The authors of the chapters were commissioned in 1982 by the United States Information Agency, the Georgetown University Center for Strategic and International Studies, and the Kennan Institute for Advanced Russian Studies in Washington, D.C. to present papers, which were exposed to vigorous discussion during a series of four one-day conferences held in 1983. Subsequently, the authors revised their papers, which appear in this book in their final form. This whole process was initiated by the policy makers in Washington, who were eager to benefit from experts in the field of Soviet studies. It is very encouraging that government officials and policy makers are anxious to cooperate on an intellectual level with outstanding scholars. This work is of such quality that it will also be useful to academicians at large who want to keep up with the latest developments in the Soviet Union.

The verdict of the experts is that the Soviets have not found a magic solution to their archaic and ailing economic system, and that reforms have encountered resistance from the rigid and inefficient bureaucracy. There appears to be no prospect that Soviet domestic and foreign policy will change substantially under Gorbachev. None of the authors implies that Soviet dictators will become Jeffersonian democrats. Like his predecessors, Gorbachev undoubtedly will make every effort to eliminate fear of Soviet aggression and to confuse and divide Wstern sentiment about long-term Soviet intentions. Although there has been a rapid succession of general secretaries since Brezhnev, the fundamental nature of Soviet totalitarianism remains the same. Contrary to wishful thinkers in the West, the Soviet regime does not shed its police-state characteristics; it dies when power is wrenched from its hands.

This reviewer's only regret is that Solzhenitsyn, Bukovsky, or other distinguished refugees from the Soviet Union were not invited to contribute to this book. Their enormous first-hand knowledge of the inside workings of the Soviet Union would have added luster to the high quality of the book. The calibre of Dimitri Simes's chapter is perfect proof that gifted and knowledgeable Russian immigrants have a more realistic grasp of American-Soviet relations and prospects for genuine coexistence than even the most able outsiders.

EDWARD J. ROZEK

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Political Tolerance on Context: Support for Unpopular Minorities in Israel, New Zealand, and the United States. By John L. Sullivan, Michal Shamir, Patrick Walsh, and Nigel S. Roberts. (Boulder, CO and London: Westview Press, 1985. Pp. xv + 264. $20.00.)

In *Political Tolerance in Context*, John Sullivan, Michal Shamir, Patrick Walsh, and Nigel Roberts continue a line of research that Sullivan started with James Piereson and George Marcus in a series of publications analyzing political tolerance in the United States. Readers familiar with that earlier research will recognize the methodology, as well as many of the same conceptual and theoretical arguments, in this latest effort comparing political tolerance in the U.S., New Zealand, and Israel.

The heart of the book is devoted to a careful analysis of survey data collected in the United

States in 1978; in Christchurch, New Zealand in 1980–81; and in Israel in 1980 (only Israeli Jews are analyzed). The authors used a "content-free" question format to measure levels of tolerance in these countries. Respondents were asked first to identify the groups in their society they disliked the most, and then to indicate if they would concede a number of basic civil liberties to members of these groups. Both New Zealanders and Americans target a variety of left-wing and right-wing groups, while a large majority of Israelis focus their antagonism on left-wing groups, especially Communists and groups that support the PLO. New Zealanders, it turns out, are more willing than both Americans (who rank second) and Israelis to extend basic liberties to the groups they dislike, largely because they feel the least threatened by their outgroups.

Surprisingly, in light of the varying degrees of threat posed by outgroups in these countries, the authors conclude that New Zealand is the most tolerant of the three nations. This conclusion appears to violate the original logic of the content-free method. When Sullivan, Piereson, and Marcus introduced this method to study changes in political tolerance in the U.S., they contended that existing studies of change had erred by re-asking present-day Americans a series of questions about Communists that were originally asked in surveys conducted in the 1950s. Because Communists had become less frightening to Americans, they argued, it was now necessary to ask about outgroups that posed as much of a threat to Americans today as Communists did during the Cold War. However, if we follow this logic, we also cannot say that New Zealanders are more tolerant than Americans and Israelis. Just as current Americans fear Communists less than the 1950s generation of Americans did, so do New Zealanders fear their target groups less than Israelis and Americans fear the respective target groups they identify.

In any event, inconsistencies aside, neither the earlier nor later logic behind the content-free method rescues this approach from being a poor measure of the level of tolerance in a society. Knowing the proportion of citizens in a society that will tolerate their least-liked group tells us little about how much tolerance prevails in that society. For example, if the citizens of one country tolerate a range of groups from A to G on the left-right continuum, but not least-liked group H, while the citizens of a second country are intolerant of all groups from A to G in addition to being intolerant of least-liked group H, it is obvious that the first country is more tolerant than the second. Yet, according to the content-free criterion, the two countries share similar levels of tolerance. The content-free method falters because it summarizes the level of tolerance in a society on the basis of an extreme data point. Instead, we should examine the range or variety of groups in a society to which people are willing to extend basic civil liberties.

Despite this conceptual and operational problem, Sullivan and his colleagues provide a valuable analysis of the social, psychological, and political factors that contribute to tolerance of the most unpopular groups in the U.S., New Zealand, and Israel. The model they develop specifies that an individual's level of tolerance is the product of four major factors: degree of support for democratic norms, personality, political ideology, and perceptions of the target group. Demographic factors such as education, social status, age, and religion operate largely in the background by influencing the development of political ideology and various personality characteristics.

In their data analysis, the authors show that personality factors have a consistently strong impact in all three nations; flexible, self-confident individuals are more tolerant than those who are dogmatic and lacking in self-esteem. Similarly, the perception that the target group is threatening greatly inhibits the expression of political tolerance.

The effects of ideology and democratic norms vary cross-nationally. For instance, political ideology has a stronger impact on tolerance in Israel than in the U.S., largely because in Israel, the effect of ideology reflects not only the positive effect of liberal norms, but also the somewhat greater sympathy that liberals, relative to conservatives, have for the predominantly left-wing target groups.

General democratic norms and specific legal norms have a much weaker impact in Israel than in New Zealand and the U.S. The authors suggest two reasons for this discrepancy: first, the concept of minority rights has, historically, been less honored in Israel than in either of the other two nations; and, second, the precarious life circumstances of the Israelis makes even those who are strongly wedded to democratic norms reluctant to tolerate the groups they fear.

All told, the authors develop a strong, well-organized argument about the variety of influences that affect the development of political tolerance. They are attuned to significant differences in the political and social circumstances of the United States, New Zealand, and Israel, and to the effect these variations will have on the structural coefficients of their causal model. The data analysis is sophisticated and well documented. Although it is based on a problematic measure of political tolerance, the book is valuable and instructive.

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Workers, Participation, and Democracy: Internal Politics in the British Union Movement. By Joel D. Wolfe. (Westport, CT: Greenwood Press, 1985. Pp. xii + 258. $45.00.)

Ever since Michels argued that democracy was impossible in large-scale organizations, critics have sought to show that it is not. These critics reject Michels's iron law of oligarchy, as well as what Gouldner called the "metaphysical pathos" of pessimism and fatalism that accompanies it. Nor did attempts to save democracy by redefining it as elite competition satisfy these critics. If this was democracy, it was hardly worth defending. Rather, these critics argued that a participatory democracy was possible in which members set agenda and held leaders accountable, instead of being manipulated by them. This is Wolfe's task—to argue that "under certain circumstances, leaders are compelled to act on behalf of their membership rather than to exercise power over them" (p. 17).

Support for the theory of participatory democracy that Wolfe presents in this book is based on his analysis of the policy process within the British trade union movement as it debated two issues toward the end of World War I: a reconstruction program for industry and policy towards the war. In each case, Wolfe claims that the union membership was able to articulate its own position on these issues, and compel the leadership to comply with it.

Wolfe argues that collective interests among workers can form as a result of forces from outside the union, and thus be beyond manipulation by the union leadership. He locates these outside forces in the workplace, which can engender collective interests among workers under certain conditions. These conditions include the ability of workers to exercise job control that permits informal workgroups to form; job security due to full employment, which gives workers bargaining power; increasing differentials between wages and profits, which stimulate feelings of injustice and exploitation; and state repression that politicizes and standardizes workplace grievances. These conditions create shared interests among workers, who then make their support of the union leadership conditional on the leadership's ability to satisfy the externally derived interests of the membership.

This, to a large degree, is what Wolfe found with regard to the relationship between union members and leaders in Britain during World War I. Led by shop stewards that emerged from the direct democracy of workers on the shop floor, skilled workers resisted conscription, fought dilution in which lower-skilled workers entered craft jobs, demanded that profits be stabilized if wages were to be restrained, and supported peace-by-negotiation to end the war. In each instance, members were able to force union leaders to respond to these demands from below.

The context of democratic theory in which Wolfe analyzes the internal politics of British trade unions is original and provocative, but his interpretation of the events upon which his theory of participatory democracy is based troubles me. First, Wolfe claims that British workers experienced a decline in living standards during the war, when his own data (Table 3.2, p. 61) shows this was not the case at all. While average weekly wage rates did indeed slip noticeably behind retail prices during the war, average weekly earnings did not, and it is the latter, not the former, that determines purchasing power. The war also squeezed wage differentials between skilled and unskilled workers. This leads to my second criticism of his analysis: the extent to which Wolfe portrays the shop stewards' movement in Britain as a class-conscious movement rather than a defense of craft privilege and exclusiveness. The shop stewards' movement was based among skilled workers, who felt that the way the wages of unskilled workers were catching up was as unjust as the way profits were racing ahead. The dilution issue was theirs, and not the unskilled workers'.

Finally, it is not clear that a special set of workplace conditions is enough to create a shared standard among workers, against which they can measure the performance of their leaders. If the workplace was so determinative of interests, why didn't the shop stewards' movement receive broader support than it did, even among skilled workers? For example, why wasn't the shop stewards' movement among engineers as successful in Barrow as it was on the Clyde? Workplace conditions were similar in each locale, but skilled engineers exhibited different degrees of militancy. Other factors need to be included to account for the militance of some workers that were transmitted up the union hierarchy during the war.

Despite these criticisms, Wolfe's blend of history and theory demonstrates how powerful the two can be when they are used to enrich each other.

ALAN DRAPER

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The Rise and Decline of the Zairian State. By Crawford Young and Thomas Turner. (Madison and London: The University of Wisconsin Press, 1985. Pp. xix + 500. $37.50.)

The authors' aims are typically discrete:

> Through the prism of the state . . . to compose a portrait of politics in the Mobutu era, from 1965 to 1980. . . .
>
> . . . to provide insight into Zaire in the Mobutu era, and [make] a modest contribution to comparative understanding of the contemporary state. (pp. 6, 46)

Nevertheless, readers will recognize this excellent volume as a major contribution to the study of politics in Zaire. Its broad scope, careful research, and meticulous attention to empirical detail make *Rise and Decline* a worthy successor to Young's earlier and equally comprehensive treatment of politics in pre-Mobutu Zaire (*Politics in the Congo*, Princeton University Press, 1965). Clearly and elegantly written, this lengthy book will amply reward those who read it. Its chapters provide a wealth of information and analysis on the Zairian polity, society, and economy during the Mobutu years.

The authors discern two distinct cycles of rise and decline between 1885 and 1980. The first ascendant period coincides with the construction of the colonial conquest state, which begins in 1885 and lasts until 1958. The initial period of state decline starts two years prior to independence in 1960, and ends with Mobutu's seizure of power in 1965. Then follows a temporally telescoped repetition of the cycle under Mobutu. The years 1965 to 1974 witness a period of reconstruction and expansion of the Zairian state along all dimensions, which, in turn, leads to a second era of dissolution and decay (p. 7). Although they review the first cycle, the authors devote the vast majority of their attention to the Mobutu years. Throughout their narrative, which treats not only the rise and decline of the state, but also the state's relations with civil society in each of these phases, the authors maintain that

> the state itself is the most decisive single factor in the complex pattern of mutations and reformulations of social consciousness within civil society. The nation-state . . . provides the framework, context, and situation which shape the particular class and ethnic configurations at any given moment. (p. 162)

Young and Turner skillfully weave astute political analyses into their tale. Especially noteworthy is their explanation of why the Mobutu regime has not given way to a major social upheaval, despite its less than exemplary record in managing the economy, its inability (and perhaps unwillingness) to decrease the widening gap between rich and poor, its failure to stem the enormous erosion in the standard of living, its cavalier view of human rights, its inability to guarantee the state's territorial boundaries (Shaba I and II), and its extraordinary corruption. Their response to the paradox of Mobutu's durability entails a discussion of the still-pervasive myth of educational mobility; the widespread belief that external powers guarantee the regime; the increasingly important survival mechanisms of the informal economy (p. 137); and the presence of a system of kinship-clientelage that sustains the hope of eventual social mobility for the lower classes (p. 160). To these, I would also add the very real fear of violent retaliation by the state's various coercive arms.

Despite this work's substantial merits, two critical comments need airing. First, a quibble. I am not persuaded that the second decline begins in 1974. If we accept the authors' main premise concerning the cyclical nature of the state, it is still possible to argue that the decline begins somewhat earlier. To be sure, by 1974 it had become visible for all to see, but I suspect that it actually began in either late 1970 or early 1971, when the relative financial health engendered by the currency reforms of 1967 began giving way to the budgetary and financial manipulations necessary to underwrite Mobutu's grandiose vision of Zairian development. It also became clear at about that same time that Mobutu would not honor his pledge to relinquish power after five years. Moreover, if we reject the main premise, could it not be argued that both periods of postindependence decline result from a single systemic crisis spawned by the nature of the colonial experience and capitalist penetration?

Second—and this is a more serious failing—Young and Turner's theoretical framework does not do justice to their fine political analyses. In chapter 1, the authors review various definitions of the state and find those of Pogi, Skocpol, and Dyson especially impressive. Yet they neither embrace one of these nor offer their own formulation. They maintain that "the complexity of the state as a concept makes impossible its reduction to a single dimension, or its definition in a parsimonious phrase" (p. 12). Instead, the authors list a series of essential attributes, or defining characteristics—territory, sovereignty, nation, a set of institutions of rule, a legal system, and the state as an idea which exists in the minds of officials and citizens (pp. 12–13)—that, when coupled with a list of the state's behavioral regularities—the imperatives to maintain territorial hegemony, uphold and advance state security and independence, maintain some form of political legitimacy, and provide revenue (pp. 15–16)—are supposed to provide a framework for understanding the state.

The difficulty is that these lists of attributes and regularities are functional requisites rather than a definition. Young and Turner appear to have sought the essence of the state and found an answer based on what it does during periods of ascendancy. While in decline, however, states neither perform all these functions nor display all these attributes. If this is the case—and the authors' subsequent analysis shows convincingly that it is—then which are the key defining characteristics of the state? Which are necessary *and* sufficient conditions for what J. P. Nettl once termed "stateness"? When, according to these criteria does a state stop being a state? And what does it then become? The question of necessary and sufficient conditions also becomes important if one wishes to examine the state in historical perspective. Their list of defining attributes is clearly influenced by European history. While this European orientation complements the importance of the colonial state, it tends to ignore other forms of rule that evolved in precolonial Africa. Using these definitional criteria, would the authors have recognized states in precolonial Zaire, where kingdoms and empires also arose and declined? Probably not, and this reduces the utility of their perspective for comparative analysis.

The authors' functional framework successfully handles the state in its periods of ascendancy, but seems inadequate for periods of state decline. This is most unfortunate, because while correctly recognizing that most theoretical visions assume, whatever the functions of the state, that states somehow rationally and systematically relate means and ends, Young and Turner demonstrate conclusively that this is no longer true in Zaire during periods of decline:

> In Zaire, the central feature is in the shrinkage of the competence, credibility, and probity of the state. It has progressively lost its capacity to relate means to ends, bringing about a loss of belief, within civil society, that the state can be expected to perform its accustomed functions. (p. 45)

The question of state decline, like others the authors raise, will require more reflection and further reformulation of our understanding of the contemporary state.

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INTERNATIONAL RELATIONS

Between Washington and Jerusalem. By Wolf Blitzer. (New York: Oxford University Press, 1985. Pp. xii + 259. $15.95.)

The Future of American-Israeli Relations: A Parting of the Ways? By James Lee Ray. (Lexington: University Press of Kentucky, 1985. Pp. ix + 157. $16.00.)

Here are still two more works devoted to American-Israeli relations, both readable and useful. Both were published in 1985, but presumably begun a couple of years earlier. Hence, not surprisingly, both reflect many of the questions about the relationship between the two countries that accompanied the 1982 Lebanon war and its aftermath. Aside from that point of convergence, the two works provide a vivid contrast, and reinforce this reviewer's impression that almost all works on the Arab-Israeli conflict must—if they are to make sense—be interpreted according to the sheer viscera of their authors. Yes, there are degrees and gradations, but we are virtually all either for or against Israel, and such sentiments rarely change in response to new facts or circumstances. One can think of few other subjects where sentiments (phobias, obsessions) drive analysis to the same degree, whatever the scholarly or analytical veneer.

Blitzer, author of *Between Washington and Jerusalem,* is the Washington correspondent for the *Jerusalem Post,* an American fluent in Hebrew, and a Washington insider who also has contacts in the highest echelons of Israeli politics. He also claims a personal role on the route to Camp David as the result of a question posed to Anwar Sadat at a news conference. His political sympathies reflect the ideological position of the *Jerusalem Post,* and appear to be those of a Labor Party moderate or of the moderate left in the U.S.: he is very sympathetic to Israel and is concerned about its survival. His book is very well written and laced with interesting anecdotes, personal portraits, and vignettes. His main strength, again, is his intimate knowledge of the Washington scene. He covers the Washington bureaucracy (primarily the State Department and its crucial bureau of Near Eastern Affairs; the Israeli presence in Washington (primarily the roles of several recent Israeli ambassadors); strategic cooperation; the CIA-Mossad connection; Congress; the political role of American Jews in relation to Israel; the news media and think tanks; business, labor, blacks, and Christians; and the crucial figures of Henry Kissinger, Jimmy Carter, and Ronald Reagan. This is a quite comprehensive volume, a *tour d'horizon* of the past and present relationship between the U.S. and Israel.

Generally, Blitzer sees this relationship—with all its ups and downs and despite the recent trough of 1982—as stronger at present than in the early years of Israel's existence, though also rather consistent since 1967. What stands out amid the blizzard of detail is the stark, vivid portrayal of the division between pro- and anti-Israeli individuals and institutions—you can't tell the players without a program, indeed! The reader gets a real panorama of Israel's friends and enemies—it's all here between the covers of one book. There are few surprises—although this reviewer was surprised to read that Secretary of State Marshall's anti-Israeli animus had extended as far as an open threat to vote against Truman in the 1948 election if the president went ahead and recognized Israel.

Blitzer is not, of course, the first commentator to point out that pro- and anti-Israeli sentiments and/or policy preferences do not measurably correlate with broader political ideology or with party or institutional identification. There are supporters and critics of Israel among liberal and conservative Democrats and liberal and conservative Republicans alike. There are ex-CIA chiefs, flag-rank military officers, black leaders, and even conservative think tanks on either side. However, Blitzer's own, albeit mild, political biases may have resulted in a few shadings of perspective. From this (neo-conservative) reviewer's perspective, he appears to understate the depth of the Black-Jewish problem as it applies to Israel. He also appears overly, stubbornly suspicious of fundamentalist Christians' support of Israel, and seems to try a bit too hard not to see the powerful animosity against Israel that exuded from Jimmy Carter and much of his entourage. (Ask Mayor Koch!) Blitzer's book was published, incidentally, before the now startling (if it is durable) shift of Senators Jesse Helms and Steven Symms to a pro-Israeli orientation, a switch of potentially major political import.

In contrast, *The Future of American-Israeli Relations* is a more self-consciously academic, though—somewhat paradoxically—also far more prescriptive in tone and intent. It is a lengthy essay in which the author gropes ambitiously for a solution to the core of the Middle Eastern conflict. In the process, he tries to find policy applications for some of political science's empirical theories. Bruce Russett and Paul Huth are invoked in connection with possibilities for a U.S.-Israeli defense alliance, and some theoretical literature on nuclear proliferation is called upon for answers as to whether Middle Eastern proliferation would or would not promote stability or peace.

However, I must admit to some bafflement about exactly what Ray is up to in this book. The author himself confesses dilettantism in conceding that he is not a specialist on the Middle East (there is no information about whether he is well traveled or well connected there). No doubt it is a bit out of bounds to do so, but I'll speculate about some of his purposes.

The tone of the book is unmistakably pro-Arab, though the author often issues—perhaps revealingly—virtual disclaimers. One has the impression that despite its aspirations of being "utilitarian" (do the best we can for both sides), and despite the previously noted efforts at applying social science concepts and frameworks, the book is somehow a very personal statement, even a catharsis of sorts. The author often appears to be addressing himself as audience. I take him to be a long-time, visceral Arabist, but one somehow so uneasy about it as to have written almost by way of expiation, if not of mere explanation.

Ray cursorily but effectively reviews the history of the conflict at the outset. That brief history will make you happy or sad, depending upon which side you are on. The Israeli Gaza raid in 1955 is seen as virtually naked, unprovoked aggression, not as a response to repeated fedayeen raids. Eisenhower and Dulles are applauded for leaning on Israel after the 1956 war. One would almost never suspect that the West Bank, including East Jerusalem, and Gaza were under Arab control for 20 years preceding 1967, and that peace did not result therefrom. The Israeli lobby looms large, though there is the generous concession that its power may have been overrated. There are the telltale quotes from long-time Arab supporters William Quandt and Seth Tillman, and an acknowledgment of the advice of Arabist Augustus Norton.

Generally—and in contrast to Blitzer—Ray sees American support for Israel as having waned after 1982, or at least having become more fragile or ephemeral. In that connection, however, the meaning of the book's subtitle, *A Parting of the Ways,* is not altogether clear. It is surely not the major theme of the book. Is it, rather, a wish-fulfillment fantasy?

At the heart of the book are its chapters on "Alternative Futures for the Administered Territories" and "U.S. Relations with Israel: What To Do." Under the former are sections on "Autonomy for the Territories," "The Jordanian Option," and "An Independent Palestine on the West Bank and Gaza"; under the latter are discussions about "Forming an Alliance with Israel" (not feasible), and "Introduction of American Troops" (also not feasible), and—curiously so in this context—"A Nuclear Middle East" as an American option. In a way, the author's presentation of the latter options must reveal his unstated assumption that the formation of a Palestinian state would not bring peace; rather, Israel is required to devise dramatic new policies to sustain its survival.

Above all, Ray is very determined that Israel relinquish the West Bank and Gaza for some sort of Palestinian state. In the manner of George ("let's make Israel save itself") Ball, he concludes his book as follows:

> Such well-known Israelis as Abba Eban see quite clearly the direction in which Israel is heading: "If we were to hear that some people in Holland were eager to incorporate 4 million Germans into their country against their will, we would conclude not that the proposers of the idea were patriots, but that they were demented. Yet the idea that Israel can permanently impose its authority over a foreign population that constitutes 30 percent of its own size is solemnly put forward as if it were a rational alternative. . . . There are two nations, not one, in that area, so that any unitary political structure is bound to be coercive, artificial, and morally fragile.

> Such an argument might be labeled "pro-Arab," but coming from a person of the stature of Abba Eban it cannot reasonably be called "anti-Israel." For the sake of Israel, and for the sake of the future of American-Israeli relation, I hope that Israelis who share Eban's views on the future of the occupied territories will prevail. They may, with a little help from their friends.

Politically and psychologically, that is the book's gravamen, particularly when it comes to the threatened "help."

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NATO in the 1980s: Challenges and Responses. Edited by Linda Brady and Joyce P. Kaufman. (New York: Praeger Publishers, 1985. Pp. xi + 274. $39.95.)

Most edited volumes on policy issues have a number of characteristics in common. They often are based on papers presented at a conference held a few years back; they frequently suffer from the lapse of time between preparation and publication; and they usually include at least a few valuable contributions. This volume is no exception.

The collective expertise of the contributors is impressive. The group, led by Linda Brady, includes a number of present or former Pentagon officials and a mixture of scholars from outside the government. The volume is organized with good effect into chapters on "challenges" (military, economic, and political) followed by analyses of "responses."

The book opens with two solid, politically mainstream chapters by Linda Brady. Leading to Brady's first chapter, the introduction suggests that NATO faces a "serious, multidimensional challenge" in the next several years but that "the challenge can be met, provided that members of the Alliance demonstrate once again the political will that has served NATO so well" in the past (p. 1).

Brady's second chapter focuses on the "military dimension of the challenge." Brady wisely avoids a simple bean-counting approach to the analysis and focuses instead on some political implications of the balance of advantages and disadvantages between the Warsaw Pact and NATO. She sensibly observes that even with all of NATO's deficiencies, the Soviet Union is not likely to attack "under present conditions" (p. 34). With an equal measure of wisdom, she observes that NATO countries must continue to improve its defensive capabilities in order to maintain the current deterrent standoff.

Three other chapters fill out the "challenges" section of the book. James Blaker's essay on out-of-area issues and NATO burden sharing focuses on an issue that was hot when the chapter was written, but which has cooled in the last few years. Nonetheless, his presentation provides useful background information and analysis. Robert Black's chapter on the economic dimension of the challenge is comprehensive, but is framed by assumptions notably more pessimistic than the tone set by Brady concerning NATO's future ability to deter the Soviets and to maintain economic and social peace within the coalition. Black's relative pessimism is balanced by Martin Edmonds's somewhat more sanguine presentation on European approaches to NATO. Edmonds nicely pulls together a number of themes that emerged in earlier chapters, focusing particularly on the requirement for effective American leadership. In Edmonds's view, this means leadership carefully balanced between attempts to improve East-West relations and NATO defenses.

The section on "responses" opens with Joyce Kaufman's discussion of NATO's force modernization programs. This chapter outlines NATO's strategy of flexible response and the alliance's attempt to deploy nuclear and nonnuclear forces to support that strategy. This is a good review of the relationship between NATO's strategy and force posture, but it never gets very far beneath the surface of the issue. Perhaps due to space restrictions, it does not outline the multiple dilemmas that have made it impossible to "perfect" flexible response in the past, and that will probably continue to do so in the future.

The chapter by James Clay Thompson and A. Grant Whitley is perhaps the most novel in the book. For over three decades NATO leaders have accepted the theoretical value of standardizing alliance weapons systems or at least guaranteeing their interoperability. They have failed more often than they have succeeded in accomplishing these purported objectives. The brief analysis in this chapter, apparently a summary of some much more extensive research, puts some scientific meat on the bones of the logical assumption that NATO forces would fight more effectively if they had standardized equipment. The case made by the authors deserves close attention if the allies hope to strengthen conventional deterrence in a future that promises serious resource constraints on both sides of the Atlantic.

James O'Leary, in his chapter on responses to economic challenges, argues that "NATO members are economically vulnerable in ways that imperil allied cooperation in diverse strategic and diplomatic arenas" (p. 166). O'Leary's statement of the problems may seem overly alarmist to some readers, and simply objectionable to others. Most Europeans, for example, would not buy his description of their motivations for detente as resting on "flawed" assumptions (p. 185). Nevertheless, his stress on informal consultation and collaboration rather than "grandiose designs" for dealing with economic security issues is a pragmatic approach to dealing with such problems among the allies (p. 186).

John Borawski's chapter on arms control and European security and Richard Eichenberg's on public opinion are both substantial and well-written contributions. Borawski effectively traces developments in the negotiations on intermediate-range nuclear forces, and mutual and balanced force reductions. The author is perhaps too skeptical about the potential value to NATO of these negotiations, but the checkered history of attempts to constrain the military confrontation in central Europe perhaps justifies a cautious approach.

Richard Eichenberg's chapter summarizes the results of his extensive analysis of European and American poll data on security issues. His analysis puts in perspective issues that tended to get exaggerated in the headlines of the early 1980s—spreading "pacificism" in Western Europe, the generation gap on security issues, and the European-American parting of the ways on defense requirements. Eichenberg argues that existing transatlantic differences in perspective will continue to produce frictions over alliance policies, but he finds no cause to believe that the alliance's base of support is in immediate danger.

The editors chose to wrap up the volume with a short chapter on NATO's future. The attempt at summarization is not particularly successful. It is too brief to give fair treatment to the chapter heading or to reflect in anything but generalities the themes exposed in the volume.

In spite of the inevitable shortcomings of such a volume, this collection includes a number of interesting essays. Those gems make the book a useful contribution to the literature on the Western alliance.

STANLEY R. SLOAN

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The Regionalization of Warfare: The Falkland/Malvinas Islands, Lebanon, and the Iran-Iraq Conflict. Edited by James Brown and William P. Snyder. (New Brunswick, NJ and Oxford: Transaction Books, 1985. Pp. xi + 291. $34.95, cloth; $14.95, paper.)

The Falklands War: Lessons for Strategy, Diplomacy and International Law. Edited by Alberto Coll and Anthony C. Arend. (Boston: George Allen & Unwin, 1985. Pp. xiv + 252. $27.50, cloth; $12.50, paper.)

The three wars of 1982 (and the Grenada invasion of 1983) have spawned a number of volumes that catalogue a variety of political, diplomatic, legal, strategic, and military lessons, and describe and analyze various facets of these small but significant wars. These two volumes add to this literature, but *The Regionalization of Warfare* suffers from a number of flaws, whereas the more focused *Falklands War* represents a much more valuable addition to the corpus of studies on these subjects.

Generally, the more ambitious Brown-Snyder collection fails to cohere as an integrated work. The three cases are treated very unevenly. For example, the chapters on the Falklands focus primarily on the military lessons, while the Iran-Iraq chapters emphasize the international political, domestic, and broadly defined strategic facets of that war. Furthermore, for some inexplicable reason, the Falklands section contains two chapters on general military precepts, with considerable overlap and no evidence of a meaningful division of labor. The emphasis throughout (with several noteworthy exceptions) is on description rather than analysis, and many of the chapters evince little organization, do not flow well, and make no contributions to an enduring literature on the wars of the early 1980s.

The weakest part of the book is section 1, on the Falklands/Malvinas. The first chapter, by David R. Segal and Katherine Swift Gravino, offers some useful generalizations about organizational cohesion and other dimensions of military sociology, but then meanders into a recounting of the origins and unfolding of the events of the war (a superfluous section, in view of an introduction that sets the stage for the actual chapters very nicely). Over 9 of the 20 pages of this chapter constitute a description of the war itself. Chapters by Dov Zakheim and John F. Guilmartin, Jr. are fairly well done, but both explore the same subject, and neither will serve as a definitive treatment of military lessons emanating from the Argentine-British battles.

Section 2, on Lebanon, contains two pedestrian but worthwhile chapters, one by Stanley Sienkiewicz on the military lessons of the Israeli invasion in 1982, and the other on the interests and consequences of the war for the many participants in the Lebanese arena. The latter, by William W. Haddad, provides a lucid overview of the implications of the conflict and its outcome for Syria, Israel, Lebanon, and the other actors in the maelstrom.

Clearly, part 3, on the neglected Iran-Iraq conflict, is the most useful of the case studies (although there is an almost complete neglect of the military lessons dimension). Chapters by coauthors Jack S. Levy and Mike Froelich and by Arthur Campbell Turner both zero in on the causes of the war and provide a balanced juxtaposition of social science and more traditional discussions, respectively. Interestingly, however, Levy and Froelich conclude that the Iraq-Iran territorial dispute was a pretext and not a cause of the war, while Turner maintains that control of the Shatt-al-Arab River was the central issue in Iraq's decision-making calculus. John D. Robertson rounds out the section with a comprehensive and sophisticated account of the statics and dynamics of Soviet interests vis-a-vis the two belligerents.

Part 4 features lessons for the United States and the Soviet Union. Chapters by Donald E. Nuechterlein (on U.S. national interests) and Schuyler Foerster (on Soviet perspectives) are both excellent. Foerster, in particular, weaves together in a masterful fashion lessons on technology/tactics, sustainability, survivability, the Kremlin's view of seapower, the use of Afghanistan as a Soviet "tactical laboratory," and dilemmas for Moscow with respect to clients and conflicts.

The Falklands War traverses a broad range of topics, and does an impressive job of covering the legal, diplomatic, strategic, political, and military lessons and implications (although the military sphere receives less representation in proportion to the others). Actors ranging from the two combatants and the United States to relevant regional and international organizations are profiled in detail. Issues including Antarctica as a potential Argentine-British conflict arena, the problem of management of boundary disputes in current world affairs, the principles of collective security and peaceful change, and the salient facets of the conflict are all discussed trenchantly.

The 15 chapters are generally well written and reflect careful, solid research. They form a coherent volume, in marked contrast to too many edited collections in international relations. While a few of the contributions are sketchy, none of the chapters is weak, and most are lucid and analytically sound. The book progresses well from part 1, on international law (the only section in which there is some repetition), to a second part on diplomacy, a third section on strategic, military, and political dimensions, and a brief but unusually effective concluding chapter by Alberto R. Coll.

Several particularly illuminating chapters should be singled out for special praise. Alberto Coll's chapter on philosophical and legal dimensions provides a lucid overview of the international law dimension, as does Anthony C. Arend's discussion of "the failure of the international legal order." In the section on diplomacy, Inis L. Claude contributes an outstanding chapter on the role and impact of the United Nations. Finally, Dov S. Zakheim's chapter on strategic, military, and technological lessons is a tour de force, and represents the best single published treatment of these issues. Zakheim chronicles an array of lessons, ranging from the strategic (deterrence and defense, naval forces in contemporary crisis management, planning for low probability contingencies) and the military (the role of naval surface forces, the relevance of amphibious missions, logistics) to the technological (with an emphasis on aircraft, missile systems, and submarine warfare).

GERALD W. HOPPLE

International Information Systems

Cuba's International Relations, The Anatomy of a Nationalistic Foreign Policy. By H. Michael Erisman. (Boulder, CO: Westview Press, 1985. Pp. xv + 203. $34.00, cloth; $13.95, paper.)

Students of Cuba's foreign policy continue to struggle with the paradox that while Cuba's influence is felt well beyond the Caribbean Basin, its dependent status and membership in the Communist bloc and in the Non-Aligned Nations Movement (NAM) produces conflicting pressures that Cuban policymakers cannot always escape. Like other actors seeking to maximize security and advance core foreign policy interests, Cuba uses its political, military, ideological, and leadership assets in an integrated fashion. Its principal domestic source of policy is still nationalism. In effect, Erisman's work focuses on the nationalistic dimension of Cuban foreign policy, while the larger goal is "to trace and analyze the overall evolution of the Cuban Revolution's foreign policy, with special emphasis on the globalist phase from 1975 onward" (p. xiv). Changes and developments from 1959 through the early 1980s are examined in four issue areas: Cuban-United States relations, Cuban-Soviet relations, Cuba's Third World policies, and Cuba's activities in Latin America. The book's six chapters are "An Overview of Cuban Foreign Policy," "The Early Evolutions of Cuban Foreign Policy," "Incipient Globalism," "The Maturation of Cuban Globalism," "Contemporary Cuban Globalism," and concluding comments with an assessment of future prospects. Several speeches delivered by Fidel Castro between 1971 and 1981 are included, and are useful for reference purposes.

The book's basic theme is that "nationalistic considerations have been among the most enduring elements" in Cuba's quest for security, in its defiance of the United States, in its efforts to promote revolution in some Latin American countries, and in its occasional rebuffs of the Soviets (p. 12). Nationalism, rather than orthodox Marxism-Leninism or enduring sympathy for Soviet-style communism, is what drives Cuba's globalism, which is itself tempered by pragmatism and a realization that "objective conditions" constrain Cuba's behavior. Erisman correctly points out that Cuba's advocacy of radicalism in the 1960s can "most generally be deemed a failure" (p. 25), but that the seeds of Cuban globalism "were quietly planted in Africa in the 1960s" through contacts between Cuban operatives with African revolutionaries (p. 32). Persistence is one of the basic features of Cuba's foreign policy, even if tactical considerations mean that long-term goals are not always pursued with vigor and dispatch. In the 1970s, Cuba cultivated radical and anti-imperialist allies among governments and non-ruling but nationalist "liberation movements," hoping to gain leverage if and when some of them came to power. This was the case with the Popular Movement for the Liberation of Angola (MPLA), and the Frente Sandinista de Liberatión Nacional (FSLN) in Nicaragua. Ties between these groups and Havana go back over 20 years. In the process, Cuba engaged in what it called "military internationalism" in Angola and Ethiopia—costly commitments on behalf of pro-Soviet dictatorships that are not going favorably for Cuba.

In the 1980s, Cuba's Third World activism has been set back by the Soviet invasion of Afghanistan, by more assertive U.S. policies, and by a shifting balance of interests in NAM itself. Cuba now has a much lower profile, and Castro's ritualistic apology for the Soviet invasion of Afghanistan is a major stumbling block for Cuba's Third World entreaties. Erisman contends that Cuba's "Third World policy has tended to emphasize developmental and diplomatic assets" over military internationalism (p. 132), but this is highly questionable in light of escalating Cuban military involvement in Angola and Nicaragua. In El Salvador, the favorable trends Erisman detected in early 1984, namely, that Havana's "FMLN allies were in fact making major gains on the battlefield" (p. 142), have been reversed, and neither Havana nor the insurgents seems capable of regaining the initiative. Havana has not recovered from political setbacks in the Caribbean proper, and its credibility suffered due to its failure to rescue its Grenadian client in 1983.

Erisman argues that Havana's warm relations with the Bishop regime were largely due to the fact that Washington spurned the regime, but this view is substantially inaccurate.

Given Washington's assertive posture in the Caribbean Basin and Soviet reluctance to bankroll the Sandinistas or substantially increase support for other revolutionary movements, Havana is expected to weigh future commitments in the region carefully, and avoid "taking major risks in the area as long as the Reaganites control the White House" (p. 186). Erisman is quite critical of U.S. policies towards Cuba and throughout the Caribbean, and somewhat indiscriminately lays much of the blame for the region's troubles on Washington. Still, he recognizes that Havana pays attention to strong signals, and tends to become more prudent in its actions.

The book successfully integrates the various strands that shape Cuban foreign policy, but the emphasis on nationalism overlooks the fusion between Cuba's foreign policy and Marxism-Leninism and its striking anti-U.S. character. In other words, the distinction between ideology and nationalism is somewhat artificial. In addition, much evidence suggests that Havana's nonalignment is dubious, that its willingness to serve as Moscow's junior partner severely limits its vaunted autonomy, and that its record of "successes" has come in partnership with some of the world's most repressive and illegitimate regimes, such as that in Ethiopia. Finally, Havana's challenges to Washington are legitimate exercises of its sovereignty, and are to be understood as expressions of calculated statecraft, not as manifestations of political altruism. The David vs. Goliath approach has many analytical flaws, and part of the argument of *Cuba's International Relations* rests on a less-than-convincing assumption regarding the *fidelistas'* infallibility.

JUAN M. DEL AGUILA

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Politics and Policy in the European Community. By Stephen George. (New York: Clarendon Press, Oxford University, 1985. Pp. xi + 197. $24.95, cloth; $10.95, paper.)

Discussion of the European Community on both sides of the Atlantic has tended to be dominated, in one way or another, by the ghosts of disappointed European federalists. Neofunctionalism, the conceptual orientation that has dominated thinking about the evolution of the European Community (EC), has itself been a kind of closet federalism. It has held that the fundamental interdependence within and among advanced technological societies would require the integration of ever wider circles of activity until eventual political integration became irresistible. Even those who have been pessimistic about federal solutions have often appeared to be "reluctant realists," who believe that some sort of federal integration would be rational and would save the future for Europe but that dark forces—mainly nationalism, fragmentation of parochial interests and short-sighted political opportunism—make such a movement toward union illusory. What has been common to nearly all those writing in this tradition has been an impatience with the slow, uneven and occasionally retrograde development of the European Community and a proclivity toward disillusionment.

Steven George has plunged into this troubled discussion with a remarkable and lucid analysis of the experience of the European movement in the light of neofunctionalist theory. Following the well-worn path of scholars and journalists who write about Europe, he poses the central question of his analysis: "Why have more common Community policies not emerged?" He attributes our expectations of more successes in creating common policy to the influence of the neofunctionalist view that there could be no turning back from integration once the logic of functional interdependence made itself felt. Since the history of the Community has proved resistant to that logic, George perceptively argues that a reevaluation and extension of neofunctionalism is needed, rather than a retreat into an empiricism without theoretical focus.

His analysis begins with an apt and admirably succinct summary of the development of the European Community. In a later chapter, he traces the problems of neofunctionalist conceptualizations of the process and argues for a restructured theory with four main dimensions. First, he believes that the idea of functional (technical) "spillover" derived from the ideas of David Mitrany, Jean Monnet, and others seems to work and should be retained. Second, the concept of political spillover, added mainly by academic theorists like Ernst Haas and Leon Lindberg, does not square very well with experience, according to George. Thus, it should be amended to de-emphasize the importance of interest groups and the executive institutions of the European Community as agents of integration and, conversely, to stress the importance of nationalism and the domestic political pressures that constrain the Community policy of national governments. Third, the economic assumption of neofunctionalism—that growth is the normal condition of advanced economies and that the benefits of growth are evenly distributed over time—George argues must be abandoned in favor of an understanding of economic cycles and of their differential impact upon economies. Finally, neo-functionalism must come to grips with the powerful influence of the international political context upon Europe including the breakdown of the U.S.-dominated global economic system, the rise in the economic salience of the Third World and of Eastern Europe, and the changed balance of power between the U.S. and the European Community.

George's analysis of specific policies of the European Community occurs against the background of the analytical approach he has developed. He selects six major policy areas: energy, agriculture, economic and monetary union, regional policy, enlargement and external relations. In each case, he brings to bear a discussion of (a) domestic political forces, (b) economic changes and the uneven impact of economic stresses, (c) the economic interests structured by the world economy, and (d) the opportunities and constraints imposed by the shifting balance of power. In spite of a tendency to offer his interpretations are uncontested facts, George succeeds in making a strong case that the shortcomings of neofunctionalist explanation are largely overcome when domestic and international political and economic factors are systematically combined in the analysis.

The curious thing about this excellent study is that, having pointed the way toward a restructured neofunctionalism, it does not actually return to the theoretical problem of accommodating the empirical dimensions it surveys. George's analysis appears to suggest the possibility of a "situational theory" of functionalism in which the operation of functional logic is located within a structure of key influences. But he does not develop the idea and may not even intend it. Instead, the focus is upon the impediments to integration imposed by conflicting national priorities. Occasionally international pressures are seen to force a common approach, as with the European Monetary System or the Mediterranean Enlargement, but the theoretical significance of such change is not developed nor is the total constellation of changes, so ably described in the study, assessed theoretically. The emphasis remains fixed upon the important difficulties that stand in the way of further progress. Perhaps the ghost of European federalism makes it difficult to find significance in anything except the uncompromising and rapid development of European Community policy.

An alternative glimpse of the same reality is suggested by scholars like Donald Puchala, Robert Keohane, and others, who have stressed the importance of converging expectations around a "regime" of values and procedures. Similarly, Robert Axelrod and other students of bargaining behavior have written about the durability of successful systems of cooperation. When applied to the European Community, such conceptual orientations emphasize the significance of the steady elaboration of institutional arrangements, the routine accumulation of mutual experience, and the anticipation of a shared future. Through such analytical prisms, the array of partial common policies in the European Community and the development of its institutional capacities look like more substantial accomplishments. At least, they suggest the possibility of a different balance in the evaluation of the European Community.

All this does not diminish the importance of this valuable analysis. It is a major contribution to the literature on the European Community and one likely to be read with delight because of the clarity, economy, and sharpness of organization with which it has been written.

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Banks, Petrodollars and Sovereign Debtors: Blood from a Stone? Edited by Penelope Hartland-Thunberg and Charles K. Ebinger. (Lexington, MA: Lexington Books, 1986. Pp. xii + 193. $27.00.)

Synergy, a word of the 1970s, has gone out of fashion. This volume does nothing to rekindle the concept. Each of its seven essays has something to say, but together they are an ill-matched lot. The book consists of an introductory review essay, two suggestions for international economic reform, and four case studies of three countries' economic problems. The essays do not hang together, and few patterns and no theory emerge. In short, the essays are better than the sum of the parts. Even the title is misleading. "Petrodollars" are mentioned twice in the introductory essay, and then never again.

Economist Penelope Hartland-Thunberg starts with a concise review of the causes and the consequences of the world debt crisis. Debt problems are blamed mostly on the recession that followed the "totally avoidable" second oil price shock. The developing countries were devastated by higher prices for their oil imports, by sharply rising interest rates, and by sagging export income.

Before launching into the case studies, two old pros suggest ways to defuse the debt bomb. Nathaniel Samuels, an investment banker and former undersecretary of state, harks back to Keynes's original vision of a strong IMF and World Bank working together "to provide the leadership necessary for encouraging sustained and consistent economic and financial policies on the part of the member countries" (p. 37). Specifically, he urges that the best way to deal with the international debt crisis would be to authorize a new allocation of special drawing rights to provide new liquidity to debtor countries. He suggests that the dangers of reigniting inflation and undermining the resolve of debtor countries to take stern domestic steps are manageable. Next, long-time congressional staffer Jane D'Arista places the debt problem in a monetary context. She contends that the current monetary system is outmoded and inadequate. She argues that to escape the debt crisis, the monetary system needs to be revised to retain a reserve currency system while expanding the number of usable currencies. Both of these essays provide interesting suggestions, but neither writer focuses on the problems of implementing their ideas.

The next three essays focus on Mexico and Brazil. First, Hartland-Thunberg and energy expert Charles K. Ebinger focus on Mexico's recent debt woes. Hartland-Thunberg then assesses Brazil's recent economic reversals and Ebinger contributes an analysis of the role of the energy sector in Brazil's economy. The first two of these essays provide thorough, blow-by-blow histories of the events leading to debt showdowns and the maneuvers and responses by the countries, their creditors, and the IMF. They provide excellent introductions to the unfolding dramas through early 1985, based almost entirely on reporting from the financial and economic press. No new data is provided, and there is minimal explicit comparison of the two cases. The essay on the Brazilian energy sector is informative, but only loosely tied to Brazil's debt problems. (Obviously, greater domestic energy production would reduce Brazilian dependence on imports and improve its debt situation.)

The final essay by Middle-Eastern expert Joyce R. Starr focuses on Israel's economic problems and what might be done to minimize them. An interesting analysis is provided that stands well on its own, but bears little relationship to the other essays. Israel's external debt situation is only one of many topics covered, and differs from the situation in Brazil and Mexico in that only about 25% of its external debt is held by commercial banks. Starr's essay and the book conclude with a famous Ben-Gurion punchline, "I always counted miracles in my calculations," and a retort that future economic miracles for Israel and presumably other debtor countries will have to be man-made. This is true, but we need better-integrated studies to solve these problems.

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Small States in Europe and Dependence. Edited by Otmar Holl. (Boulder, CO: Westview Press, 1985. Pp. iii + 341. $25.00, paper.)

For some American political scientists it has become fashionable to view approaches to comparative politics and international relations that emphasize dependency matters as being "worn out," suspicious leftist carping, or methodologically soft analysis. For others, the issues of primary concern in the international political and political economy arenas have generally to do either with military/strategic issues or with the multifaceted interactions between the superpowers. To those who have not forsaken an interest in dependency questions or those for whom the world is larger than crisis-generated and powercentric analysis, *Small States in Europe and Dependence* (also referred to as the "Laxenburg Papers") edited by Otmar Holl are a needed breath, perhaps even a gust, of fresh air.

The papers that form the volume are revisions of those presented at a session of the Austrian Institute for International Affairs held at Laxenburg in 1981. In his introductory essay, Holl points out that big states and "big brother" analysts were not included, so that the focus of attention could be kept upon the issues of smaller states (European, "Second World" ones). Holl and his associates are definite in their opinion that the directions and subject matter of research in big states overlook both the existence of the problems of the small state political economy, and the important lessons to be learned by studying its dynamics. While excluding what are perceived to be the necessarily hegemonic foci of "First World" analysts does not in any sense mean that what the contributors have to say is correct, it does present us with an important challenge. Perhaps a bit of introspective reflection might just turn up the sort of biases these folks have asserted. Certainly, we ought not allow a mix of nationalistic pride and professional hubris to prevent us from thinking about things!

I appreciated this collection's concern with political economy issues. The numerous essays address the vitally important matter of the degree to which economic dependence influences the political options available to decision makers in small states. The overall conclusion is that the impact of dependency is considerable.

However, there is more here than material for students of the several smaller Western European states. The spirit of the essays is captured nicely by Otmar Holl and Helmut Kramer in their discussion of the process of internalization and Austrian policymaking. First, they make the point that internalization is the absorbing condition of most contemporary political economy activities. Second, internationalization implies the dominance of the capitalist social mode. Finally, internationalization has rendered "international politics more dependent on economic processes while on the other hand politicizing international economic relations on a global scale" (p. 209).

For those who find the continuing description of difficulties and problems too hard to take, the volume is rich in prescriptions. For example, Benno Signitzer and Hans Heinz Fabris address themselves to various possibilities that exist as counterdependency strategies pertaining to the Austrian communications industry. Notwithstanding their pessimism about the necessary changes being implemented within their specific arena of concern, the authors represent the volume's uniformly tidy manner of blending the description of structural factors and purposive policy dilemmas.

The individual essays are written well and the arguments are developed into cogent statements. The reader will benefit from the volume's extensive documentation and the inclusion of considerable bibliographic materials. This volume is a valuable contribution to the literatures of both international relations and the politics of development. It is made the more valuable by the vantage points of its contributors.

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World Futures: A Critical Analysis of Alternatives. By Barry B. Hughes. (Baltimore & London: The Johns Hopkins University Press, 1985. Pp. xii + 243. $25.00, cloth; $10.95, paper.)

Authors are not always responsible for the titles given their books. I have no idea who decided to call this book *World Futures: A Critical Analysis of Alternatives*, but the title, *World Futures*, has already been used for a similar, but generally superior, volume produced by the Science Policy Research Unit (SPRU) at the University of Sussex and published in the U.S. in 1978. Even so, the title did not prepare me for the actual contents of Hughes's book. Rather, as one might expect knowing the author's previous work, the book is primarily a summary and discussion of several very well-known reports based more or less directly on recent global modeling efforts: namely those commissioned by the Club of Rome—*The Limits to Growth, Mankind at the Turning Point,* and the Bariloche model—Wassily Leontief's *The Future of the World Economy,* the Organization for Economic Cooperation and Development's (OECD) *Facing the Future,* and Gerald Barney's *The Global 2000 Report to the President.* Some attention is also given to Lester Brown's *Building a Sustainable Society,* Alvin Toffler's *The Third Wave,* Herman Kahn's *The Next 200 Years,* and Julian Simon's *The Ultimate Resource.*

While the first six of these volumes may be a fairly good sample of books derived from attempts at computer-based global futures modeling in the West during the 1970s, I would be hard-pressed to say that, along with the remaining four volumes, they are representative of serious thinking about the future of the world by different groups and cultures around the globe—and that is what the title led me to expect. True, a few other sources are mentioned in passing, but even important existing critiques of global modeling, such as the SPRU work mentioned above and many others, are essentially ignored.

Some of the chapter headings are similarly misleading. The introduction, "The Proliferation of Future Studies," is in no way at all a discussion of the many examples of serious academic attempts at future studies that have emerged around the world over the past 30 years, or of basic or applied futures research. I can assure the reader that such efforts are found throughout Eastern as well as Western Europe and in many parts of Africa and the Middle East; that the subject is impressively well developed in many Asian countries; and that there are perhaps a dozen good academic programs and scores of reputable research and consulting groups focusing on global as well as other futures issues throughout North America. Almost none of these is mentioned, much less "critically analyzed," in this volume.

Similarly, chapter 2, "Competing Images of the Future," with its purported "Review of Recent Futures Studies," reviews only the ten works listed above. The works reviewed are evaluated along two dimensions. One is labeled *political ecology* and comprises two categories: modernist and neotraditionalist. The other is *political economy* and has three categories: liberal, internationalist, and radical. When these are arrayed orthogonally against each other (and modernist and neotraditionalist are relabeled "no limits" and "limits," respectively), the studies reviewed fall within the resulting Cartesian space. For example, the Bariloche model thus lies in the "radical/no limits" sector, and Lester Brown in the "liberal/limits" sector.

The seven chapters that comprise the bulk of the book are derived from the "components of the global development system" of *Mankind at the Turning Point:* population, economics, energy, food, technology, the environment, and values and political structures. In turn, each summarizes the methods and substantive findings of the various global models relative to that component. They are as well done as the data they are based upon permit, but have a dry textbook flavor that makes for rather uninteresting reading for a person looking for information about "world futures."

The final chapter, "What Are We To Think?" is a bigger letdown. Only four and a half pages long, it does not go far in answering its question, which presumably refers not to thinking in general, but to what we are to think about the future of the world. Yet how could it do more, when the author ignores most of the futures literature and focuses on such a narrow, if admittedly important, part? What *are* we to think, indeed?

JIM DATOR

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Autonomy and Interdependence: U.S.-Western European Monetary and Trade Relations, 1958–1984. By Thomas L. Ilgen. (Totowa, NJ: Rowman and Allanheld, Publishers, 1985. Pp. x + 166. $34.95.)

This book provides a sweeping historical interpretation of postwar trade and monetary relations between the United States and Western Europe. Thomas L. Ilgen offers an often-compelling interpretation of the paradox presented by America's simultaneous pursuit of interdependence and autonomy. Immediately after World War II, the United States used its power to develop liberal trade and monetary regimes intended to foster economic interdependence. Yet it also wanted autonomy in domestic economic management. When autonomy and interdependence conflicted, the U.S. acted to change trade and monetary regimes in order to insure the maintenance of American autonomy. As a result, America's relative economic power declined, and an international political economy founded on economic interdependence is replacing one based on hegemonic power.

Ilgen presents a great deal of historical detail in support of his thesis, and this well-written book includes discussions of domestic developments in Britain, France, Germany, and the United States. He describes a wide array of international trade and monetary negotiations, and offers powerful discrete observations throughout.

The book is also theoretically eclectic. It draws from the literature on hegemonic stability, from arguments about the international division of labor, and from arguments in comparative politics about the importance of domestic economic arrangements and institutional relationships. Ilgen synthesizes existing work to develop an important and compelling historical thesis.

Ilgen presumes that autonomy and interdependence are incompatible objectives, and that the American struggle to maintain autonomy in the face of interdependence has worked to its economic disadvantage. Yet he does not define autonomy precisely, and in none of the senses in which he uses the term does autonomy necessarily conflict with interdependence. At times, Ilgen equates autonomy with protectionism. Elsewhere, he uses it to describe America's disregard of other nations, and in still other places it seems to mean the U.S. pursuit of domestic macroeconomic policies without concern for balance of payments considerations. However, autonomy can be pursued either to prevent the workings of interdependence or in full recognition of it. The protection of textiles in the early 1960s, for example, can be interpreted as the pursuit of autonomy in the face of interdependence, or as the domestic price for the further liberalization of trade and the growth of interdependence. Moreover, autonomous policies can be used either to improve American competitiveness or to shelter industries from structural adjustment.

The problem with portraying autonomy and interdependence as incompatible and with viewing the pursuit of autonomy as harmful to the United States is evident in Ilgen's analysis of the last decade. The Carter administration self-consciously embraced coordination and interdependence rather than autonomy, but the failure to achieve consensus on coordinated macroeconomic stimulation resulted in an unintended autonomy for the United States. Ilgen sees the Reagan administration, on the other hand, as returning to earlier attempts to maintain autonomy—as ignoring international consequences and concentrating solely on domestic ones. Yet it has not attempted in the name of autonomy to shelter domestic industries from structural adjustment, as had earlier administrations. Its insistence on market solutions has led it to reject both coordination and most protection. The incompatibility of autonomy and interdependence (and the presumptive need to coordinate in an interdependent world) depend on the nature of autonomous policies; autonomous policies can improve adjustment and competitiveness, just as coordination can fail.

The strength of this book is its interweaving of domestic and international economic policy and of monetary and trade policy. The historical interpretation is provocative and often compelling.

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Interdependence on Trial: Studies in the Theory and Reality of Contemporary Interdependence. Edited by R. J. Barry Jones and Peter Willetts. (New York: St. Martin's Press, 1985. Pp. x + 237. $29.95.)

I first heard the term *interdependence* as a child in a Catholic elementary school in the 1950s. Following daily Mass we would pray with the priest for the conversion of Russia, and then go off with the nuns to have the effects of our prayers for U.S. hegemony undermined by indoctrination in the concepts of interconnectedness and brotherly love. During those years of American confidence in the power of our dollars and our weapons, the Franciscan sisters were out of step with the "realism" of Cold War Catholics and their ideological brethren who dominated the way most of us viewed the international system.

As Jones and Willetts tell us in the introductory chapter of their provocative collection of essays on interdependence, events in the late 1960s and early 1970s undermined the confidence of both lay and official observers in the realist paradigm. It gave way to a world view that focused on the limits to power and the proliferation of entangling ties that prevented its rational exercise in increasingly complex and even internally contradictory national interests: "Interdependence, in this vein, may thus be viewed as a rationalization of failure in Vietnam and the enforced abandonment of dollar convertibility into gold" (p. 2). As realism makes its comeback on the coattails of the elections of 1980 and 1984, it is easier to see the flaws in both perspectives when they are used reflexively, as blunt instruments of foreign policy making and analysis.

The first four essays in this collection demonstrate very nicely that interdependence is far from being a single concept. A major analytical problem is the lack of agreement about whether interdependence is a property of states or of systems, a problem that has bedeviled those who try to measure interdependence and its changing levels over time. Like most of the measurers, Jones, in his essay, settles for interconnectedness as at least an indicator of interdependence among states, but along with Willetts and Steve Smith in their contributions, he also recognizes the empirical and perceptual complexity of the concept to both the people who study international relations and those who engage in them. Other ways to understand interdependence are also discussed, such as the equation of interdependence with a perception of a common fate, the rationale for oligopoly behavior among firms, and deterrence behavior between superpowers. The latter model informs Little's argument that realists believe in interdependence too.

The link between situation and ideology and the choice and application of an analytical framework is clearly drawn throughout the volume. It is treated theoretically in the essays already mentioned, and empirically in John Vogler's chapter on the World Administrative Radio Conference and in John MacLean's chapter on UNESCO. Michael Smith's essay on Atlanticism and interdependence in the North Atlantic is an elegant analysis of the sometimes tenuous connection between empirical reality and perceptions of system structure. All these essays emphasize the role of subjective factors in determining the content and interpretation of analytical models.

An element that I believe is insufficiently explored in this collection as a whole is the difference between the organizing principles of interdependence and hierarchical frameworks such as realism and dependency. Even though interdependence in its ideological context has been used by powerful countries to justify cooperation by less powerful actors to uphold regimes that benefit the powerful (see the arguments made by Vogler and MacLean), the quality of the relationship as viewed through the interdependence model is different than it would appear from a realist perspective. Interdependence takes sovereignty, rather than hierarchy, as its organizing principle. Bargaining rather than force is seen as the legitimate means of resolving conflicts. This is why the interdependence model can explain empirical situations such as international relations during periods of declining hegemony, or where participants perceive themselves as sharing a common fate, better than realism. If interdependence is "on trial," it is because it can't explain why bargaining breaks down or is bypassed altogether in favor of the application of force. The priests of power and the sisters of cooperation are still unable to reconcile their divergent views into a comprehensive model of how the world works.

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European Peace Movements and the Future of the Western Alliance. Edited by Walter Laqueur and Robert Hunter. (New Brunswick, NJ and Oxford: Transaction Books, 1985. Pp. xxi + 450. $45.96.)

West European Pacifism and the Strategy for Peace. Edited by Peter van den Dungen. (New York: St. Martin's Press, 1985. Pp. viii + 218. $29.95.)

History has demonstrated time and again that no alliance is eternal. The political, economic, and military environment that gave birth to NATO in 1949 has undergone a transformation so dramatic that elites and masses alike have begun to question the alliance's prospects for survival, at least in its present form. Mass protests against the deployment of Pershing II and cruise missiles in the early 1980s may have highlighted the need for a reevaluation of prevailing alliance strategies, but the erosion of consensus among American and European security partners commenced in the late 1960s and early 1970s. These years were marked by a declining confidence in the United States' capability and/or willingness to protect the Continent at the expense of its own survival, as well as by changing perceptions of the Soviet threat, based on different experiences with détente.

A 35-year emphasis on the military dimensions of the Atlantic Alliance has precluded a search for a new consensus grounded in common political purposes that would serve to rejustify its existence and lend coherence to its strategies. The search that lies ahead is fraught with complications unknown to defense planners of earlier eras, owing to the growth of an attractive, activist, antinuclear public, as well as to a reassertion of national interests among European states. In the long run, attempts to reform NATO or to replace it with an alternative security system will succeed only if three conditions are met: (1) policy makers structure their defense systems around a positive, operational definition of peace that addresses the sources of global and East-West conflict; (2) European and American partners find a concept of security that ensures an equitable distribution of costs, guarantees, and risks among all members; and (3) leaders in Western democracies undertake steps to inform, reintegrate, listen to, and learn from the successor generations constituting a major segment of the critical, mobilized, protesting public. Direct dialogue among the political establishment, defense strategists, peace researchers, and a public terrified by possible nuclear annihilation is long overdue.

These two volumes appear to offer an ideal forum for such a dialogue, but they fail, for the most part, to deliver the syntheses promised in their titles. In *European Peace Movements and the Future of the Western Alliance*, editors Laqueur and Hunter argue that Western Europe has sufficient manpower and resources, but not the political-psychological will to provide for its own defense. The link between this missing will and peace movements is not as "obvious" as they assume, however. Hyland speaks to European leaders' unwillingness to provoke an open nuclear debate, but sees strategy as nonetheless having been abdicated in favor of politics, and politics as sacrificed to public relations. Kristol's (reprinted) critique of the nonexistent nuclear umbrella and untenability of a graduated deterrence strategy ends with the warning that U.S.-nationalistic unilateralism is likely to wreck the alliance. Kissinger's (reprinted) plan for reshaping NATO stresses that an alliance incapable of agreeing on fundamental political premises cannot survive by adhering obliviously to military arrangements adopted under radically different circumstances. He recommends a European planning and command role. Draper (reprint) holds that what is new about the current crisis besetting the alliance is the questionable reliability of the West Europeans now being called upon to aid the U.S. in pursuing its own security needs. Hassner sees a potential German neutro-nationalist lurking behind every peace banner, willing to risk the division of the alliance to advance its own rapprochement cause.

Portrayers of the individual peace movements—Haagerup on the Netherlands, Ceadel on British nuclear disarmers, Gnesotto on the French front, Norman on the pacifist contributions of British churches, von Kielmansegg on the origins of the German movement—offer few new insights against the backdrop of the many campaign "autobiographies" already in print. Alting von Geusau's polemical, "Communist-plot" assault on the Dutch and Belgian movements, along with de Maiziere's facile dismissal of German pacifists' efforts at least to introduce alternative security concepts detracts significantly from the quality of the debate. Herf's acerbic essay equating neutralist aspirations with moral confusion and fractured liberal democracy in the Federal Republic of Germany (FRG) attends more seriously to the sources of protest than does Thomas's characterization of the American freeze campaign as naive, emotional, and Orwellian. Neither the more interesting propositions advanced by Scharrer on the search for new ethical standards and political activism among the German churches, nor the more concrete proposals for restructuring NATO covered at the book's outset, receive further notice—there is no critical-analytical concluding chapter.

West European Pacifism and the Strategy for Peace, on the surface, admits to an analytical distinction between the idea of pacifism, peace movements directed against specific security policies, and integrated strategies for peace. Andreski relates the search for peace to differing conceptions of human nature, and thus to different explanations for the origins of war, rejecting the idea of a killer instinct among humankind. Editor van den Dungen identifies the theoretical origins of pacifism, along with various schools and types ranging from heroic and Christian to imperialist and cultural pacifism. Comments by Shtromas quickly degenerate into an anti-Communist tirade. He equates unilateral disarmament steps with unconditional surrender, claiming that "the West's surrender to the Soviets is even more lethal than nuclear war itself" (p. 40).

The anti-red histrionics gather momentum in subsequent chapters. Unlike the essayists in the Laqueur-Hunter volume, however, these authors cite little evidence of ever having read the works of those they denounce as ideologically obdurate, "normal machiavellians" (p. 61). Clesse charges the peace movements with trying to impose neutralization through the "language of the streets" and media manipulation. Radnitzky employs a sociobiological framework for judging whether movement supporters themselves—said to prefer moralistic aggression and spiritual self-mutilation to rational discussion—constitute "pitfalls for peace." Shtromas returns to assess peace politics in the Soviet Union, where the population gives its "dissenting assent" to false promises and pseudo-initiatives in exchange for a minimum of physical security. Harmel posits that Communists use peace/pacifism to their own advantage, but also use war when necessary to advance the revolution. In addition to expanding their own propagandistic organizations, they exploit and infiltrate any and all spontaneous movements that "bleat for peace" (p. 168). Gallois writes of asymmetries threatening Western security, including the "often absurd intervention of the public" (p. 175), and urban vulnerability in advanced industrial Europe.

At least three voices of reason emerge. Tromp stresses the importance of the "paradigm shift" in security thinking that has been brought on by the paradox of deterrence: mutual assured destruction (MAD) threatens such a horrible global end that no rational actor would resort to it, thus undermining the credibility of the threat. The key to effective security policy rests in the recognition of interdependence among all states. Von Baudessin holds this to have been the aim of détente; political cooperation and binding crisis management procedures should take precedence over military capability. Morton Kaplan outlines concrete steps for reducing the risk of war through conventional "transarmament," nuclear-armed Quick Reaction forces, "dissuasion" tactics applied to the East, and parallel command rights for each ally. Klein judges this model to be more satisfactory than Mutual and Balanced Force Reduction (MBFR) proposals made thus far in Vienna and less radical than Reagan's "zero option," yet sees disarmament as but one component of a general peace policy. Von Baudessin objects to the prescription for turning "flexible response" into "flexible responsibility."

The "extraordinary array of talent" (book-jacket claim) ostensibly includes one woman among the 26 authors represented in these two books, and no activist peace researchers. The "surprising consensus" that emerges is therefore not so surprising to this reader. In matters of war and peace, "it is political power that counts; in politics power dominates arguments or rationality" (p. 69). Despite their success in raising arguments critical to the survival of the species and in posing questions as to the rationality of deterrence, the peace movements are a long way from threatening or transcending the power-political structure of the Atlantic Alliance.

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Superpower Involvement in the Middle East: Dynamics of Foreign Policy. Edited by Paul Marantz and Blema S. Steinberg. (Boulder, CO and London: Westview Press, Inc., 1985. Pp. ix + 301. $21.50, paper.)

This volume is divided almost equally between Soviet and United States policy in the Middle East. The first three chapters deal with the recent evolution of Soviet perspectives on the utility of force in the Third World (S. N. MacFarlane), Islam as a political force in the Middle East (Carol R. Saivetz), and U.S. goals in the region (Herbert L. Sawyer). These are followed by two case studies of Soviet policy: one on the Lebanese crisis of 1982–84 (Robert O. Freedman), and one on the apparent Soviet decision of 1972–73 to aid and abet Sadat's decision to go to war (Dina Rome Spechler).

Thereafter come four chapters on U.S. policy and perspectives: one on the role of oil as a factor in U.S. calculations (David Haglund); one on the assumptions of State Department Arabists about the character of the PLO (Martin Indyk); one on general U.S. policy assumptions about the nature of society and conflict in the Middle East and Persian Gulf (James R. Kurth); and one chapter that attempts a general evaluation of the Reagan administration's Middle East policy during its first term (John H. Sigler). The final section of the book offers an analytic framework for assessing the impact of the U.S. and USSR on the historical evolution of the Arab-Israeli conflict (Shmuel Sandler). Finally, a concluding chapter briefly summarizes the theme of each contribution to the volume (Blema S. Steinberg).

The virtue of the volume is that it brings together an impressive array of talent whom it allows to present research findings (more typical of the Sovietological chapters), or to make evaluative statements (more typical of the chapters on the U.S.) about the aspect of superpower involvement in the Middle East in which they are most interested. The drawback of the volume is that it has a topic, but no theme. The contributors were apparently not given a common problem to address or puzzle to solve. Consequently, the volume lacks coherence; not surprisingly, the final chapter reads more like a preface than a conclusion.

Some questions that could have been addressed by almost all contributors are the following: What would be the political feasibility in Moscow and Washington of joint action to deescalate the Arab-Israeli confrontation? Could Moscow and Washington have controlled their clients even if they had agreed on joint action? Given the turmoil in the region, to what extent is the superpowers' competitive relationship a zero-sum game? To what extent has it turned into a negative-sum game? To what extent would superpower collaboration be a positive-sum game? How have perceptions and perspectives on these matters evolved over the years in each capital? Each chapter in the book, however partially and indirectly, has something to say about some of these matters. As presented, however, there is nothing explicitly additive about the contributions.

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Guests Come to Stay: The Effects of European Labor Migration on Sending and Receiving Countries. Edited by Rosemarie Rogers. (Boulder, CO: Westview Press, 1985. Pp. xiii + 344. $25.50.)

The Unwanted: European Refugees in the Twentieth Century. By Michael R. Marrus. (New York: Oxford University Press, 1985. Pp. xii + 414. $24.95.)

Guests Come to Stay is a carefully organized and skillfully edited volume addressing an issue that has received remarkably meager scholarly attention in the United States: the political, social, and cultural challenges spawned by Europe's experiment with temporary worker programs. The volume is ambitious, in that it attempts to be comprehensive by offering both a retrospective and prospective look at the problem, and by incorporating a discussion of an often-ignored and always controversial component of the issue: the impact of emigration on the sending societies. Yet, such laudable goals notwithstanding, *Guests Come to Stay* delivers only meager results. In spite of a commendable introduction by the editor, in which she sets out very clearly the multitude of issues and tensions to which "guestworkers" give rise, there is no apparent attempt to tie the many presentations together either theoretically or substantively. As a result, the volume stands naked as a set of mostly descriptive essays that cover a number of key issues associated with European temporary labor programs, but also allow other issues of equal or greater importance to go unattended.

An example might suffice to demonstrate this point. The volume highlights one of the key challenges to receiving societies posed by the institution of foreign workers: the second generation problem. The discussion of the problem, however, is largely mechanical and often seems to get its cue more from the receiving governments' preferred perspective of "damage control" than from a genuine analytical interest in the challenges and opportunities that second-generation migrants pose for key receiving societies. That only one of the contributors to the volume, Hans-Joachim Hoffmann-Nowotny, writes of "immigrants" rather than "migrants" or "foreigners," and that none of the contributors speak of second-generation Germans, Swedes, or Swiss, highlights both the short-sightedness of Europe's policy responses to its de facto immigration situation and the lack of analytical foresight and initiative on the part of most authors. With this serious problem in mind, one must wonder if this volume is, analytically, a second-generation effort in the understanding of the phenomenon of foreign labor in Europe, or simply an updated first-generation version following in the footsteps of earlier mediocre works on the topic.

While the essays by Friedrich Heckman and Tomas Hamar attempt to go beyond the traditional questions of the literature on the topic, the lack of an organizing principle or a *mise en scène* weakens the volume even further. So do a number of other more minor but equally puzzling problems: the editor's unquestioning adoption of an explanatory framework ignoring the substantial structural forces for emigration in favor of an approach that views the decision to emigrate as "voluntary"; a drawn-out discussion of the permanence/temporariness question that ignores the view of virtually all immigrants, and even many refugees, that their emigration is essentially temporary; the positing of Austria, rather than France, as the archetype of a country that allowed foreigners to seek and obtain work and then apply for work permits; and a far too sanguine view of the degree to which "administratively enforced departures" were adhered to and became a disruptive force in foreign worker communities in the 1970s—particularly in the Federal Republic of Germany.

These problems notwithstanding, *Guests Come to Stay* is a worthwhile and relatively up-to-date account of the status of foreign workers in Europe. It contains generally competent analyses both of the situation of foreign migrants in advanced industrial Europe and of the consequences of emigration in the sending countries.

While the Rogers volume virtually ignores the entire question of refugees in Europe, Michael Marrus's *The Unwanted* focuses explicitly on that type of population movement. Marrus brings a historian's training and interests to the topic, and delves deeply into the causes of refugee generation and the responses refugees received where they resettled. *The Unwanted* tells a tale of genocide, war, mass expulsions, exchange of populations, prejudice and human suffering, dislocation for those seeking refuge and those offering it (often with great reluctance), marginality, and official ambivalence and confusion about refugees. It tells of the persistent tension between politics and humanitarianism and the frequent surrender of the latter to the former, of nation building and of the cruel calculus that accompanies radical systemic change.

Remarkably, although the volume focuses on the first half of the twentieth century, little has changed either in the urgency of the problem and the consequent need for safe havens, or in the ambivalence of the reactions of governments and people toward refugees. The birth of the predecessor to the current United Nations High Commission for Refugees (UNCHR), the League of Nations High Commission for Refugees, is well documented by Marrus, as are the financial and political limitations of the office and the commitment of the first commissioner, Norwegian Fridtjof Nansen.

There is a veritable thesaurus of information and historical insights in this volume. It covers not only the responses to and the reception of refugees by the destination governments, but also the antecedent and proximate causes of refugee flows as disparate as the many Jewish exoduses since the 1880s, the Armenian genocide, and the Greek catastrophe in Asia Minor, with its consequent mass exodus of ethnic Greeks from Turkey and the Greek-Turkish exchange of populations.

Throughout this fascinating book of government excesses and human suffering, Marrus manages to offer an even-handed, superbly documented, and clearly written analysis of each episode, while simultaneously unraveling the web of another story: the evolution of the international procedures and institutions that would act as occasional buffers, but more frequently as impartial but concerned middlemen, in refugee-generating crises. In this parallel tale one finds the best and the worst about international initiatives—the meeting of the basic survival needs for most refugees and the financial and political constraints of transnational institutions, as expressed in their inability to prevent, defuse, or ameliorate situations that create refugees. Yet, in view of the considerable successes of both the League and the United Nations High Commission for Refugees in assisting with the moving and relocation of perhaps as many as one hundred million refugees, one must be both humbled by the enormity of the task and thankful for the response of the international community in establishing and continuing the institution of the UNHCR, in spite of the extreme political sensitivity of its mission.

These two volumes are somewhat uneven in quality and are intended for different audiences, but they are quite similar in the intensity of their messages. Both deserve the attention of serious students of twentieth-century Europe. Both address important problem areas. Finally, both are eloquent advocates of the need for European states to develop systematic (preferably joint) initiatives to address continuing challenges, namely, coming to terms with their own unmistakable ethnic pluralism (the Rogers volume) and the persisting need to anticipate, and thus plan for, the orderly resettlement of refugees (the Marrus volume).

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New Nationalisms of the Developed West. Edited by Edward A. Tiryakian and Ronald Rogowski. (Boston: Allen & Unwin, 1985. Pp. xii + 394. $35.00.)

The avowed purpose of this volume is to address the "anomalous phenomenon, that nationalist movements seeking radical decentralization or emancipation from state authority arise at an advanced state of modernization" (p. 4). The theoretical foundation for the anomaly is the paradigmatic expectation that modernization and mass participation in industrial democracies would tend to eliminate ascriptive solidarities as the basis of political cleavage in favor of class or interest-group activity. The empirical basis for the anomaly is the apparent rise in "ethnonational" political activity in "advanced industrial states" during the late 1960s and the 1970s.

Indeed, it was in the late 1970s that the group whose work is represented in this book was formed. Most of the contributions are based on work that was done at the beginning of the 1980s. It would appear that by that time the nature of the "anomaly" had changed. Most of the 16 individual contributions, as well as the concluding chapter by Ronald Rogowski, are devoted to explaining why ethnonationalist movements in the developed world—especially in Britain, Canada, Spain, and France—have experienced decreasing success, and/or why theories developed to explain their emergence and growth were flawed.

Rogowski suggests a general theoretical approach to nationalist movements—he prefers not to distinguish "new" from "old" nationalism—couched entirely within a rational choice perspective. As Rogowski emphasizes again in his concluding remarks, he sees the need to reject "primordialist" as well as reactive explanations of ethnonationalism in favor of a broader approach to the study of "political cleavages" in terms of changing incentives to adopt alternative political identities.

The judgments implicit in this line of analysis—that identities are plastic and self-selected and that they are usefully considered as instruments employed by individuals to accomplish larger social and economic ends—are common to many of the essays in this volume, including both of Michael Hechter's contributions. Hechter retreats even further than he has previously from his "internal colonialism" explanation of contemporary Celtic nationalism. Adopting an Olsonian rational choice perspective, he explains the declining fortunes of Scottish and, with Margaret Levi, Welsh nationalist parties by identifying shifting selective benefits, available to Scotland and Wales from participation in the British state and to individual Scotsmen and Welshmen for supporting non-ethnonationalist parties. Jack Brand's response to Hechter emphasizes the incorrectness of the latter's original predictions about Scottish nationalism, and also takes issue with his amended version as advanced in this volume. According to Brand, Scotsmen have had even less reason to vote nationalist than Hechter realizes, since the Scottish National Party has had no patronage to offer its followers.

Mario Polèse evaluates the variables that determine the economic rationality of ethnoregional separatism. His argument, illustrated in the Canadian context, is that on balance, separation is probably more likely to be costly than profitable for peripheral regions, but that too much indeterminacy exists in the trade-offs that must be made along different economic dimensions for a strictly economic analysis to be conclusive. Polèse's analysis is consistent with that of Oriol Pi-Sunyer, whose theoretically and historically balanced contribution on Catalan nationalism in Spain refers to the protectionist policies that central states can adopt, affecting the incentive structures of inhabitants of peripheral regions and thus reducing the appeal of ethnonational or ethnoregionalist parties.

Jurg Steiner's discussion of modes of decision making in Switzerland with respect to separatist sentiments in the Jura also draws attention to the strategies and techniques available to central states in their efforts to keep peripheral regions with separatist tendencies within the ambit of central state authority. Phillip Rawkins's analysis of the cruel dilemma facing Welsh nationalists, as they seek minimal electoral success at the expense of purity in the appeals the party makes to nationalist values and ambitions, substantiates the general point that the political mobilization of ethnonationalist sentiment is in large part a function of central incentive structures controlled by the state.

The longest and the best contribution is Juan Linz's study of Basque, Catalan, and Galician political sentiments. Linz uses carefully gathered attitudinal survey data to show that ethnonational political movements, as well as individuals, adjust their thinking to changing incentive structures. His argument, largely corroborated by his data, is that as ethnonationalist movements seek more radical, political, and separatist objectives, their appeals shift from primordialist notions of what the salient identity is to regionalist, nonascriptive ideas. Only in that fashion, in relatively developed "peripheral" regions such as the Spanish Basque country and Catalonia, can the sense of exploitation felt by so many of the new immigrant workers be tapped in order to achieve political or electoral success. He shows that definitions of identity as determined by ancestry or language—as opposed to beliefs and/or place of residence—are more likely to be associated with loyalty to the central state than to an ethnoregionalist political movement.

The book is marred by numerous typographical errors. Several of the contributions are thin, and read more as impressionistic reports of research in progress than as carefully considered contributions to the theoretical and empirical problems at hand. Nonetheless, the volume as a whole is valuable and stimulating. That the answers provided to the original organizing question tend to refute its premises reflects substantial intellectual development in the political analysis of nationalism, new and old.

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Leaders Under Stress: A Psychophysiological Analysis of International Crises. By Thomas C. Wiegele, Gordon Hilton, Kent Layne Oots, and Susan V. Kisiel. (Durham, NC: Duke University Press, 1985. Pp. xvi + 228. $32.50.)

Leaders Under Stress adds a new dimension to the study of international crisis behavior. It focuses on the state of mind of leaders during crises. However, instead of using the traditional method of content analysis of the written word, it uses what the authors term "signal leakage" in the words spoken by the president during the crisis. This is done by taking three international crises—the 1961 Berlin crisis, the 1965 Dominican crisis, and the 1970 Cambodian crisis—and subjecting all presidential speeches and press conference remarks pertaining to them to voice stress analysis. This data is then analyzed for thematic stress and psycholinguistic structure.

The authors come up with some interesting results. Concerning the Berlin crisis, they discovered that the theme of reservists was more stressful to Kennedy than the themes of freedom for Berlin or of the need for a military buildup. They also report that, due to a lack of severe stress themes in remarks made after November 29, the "cognitive saliency of the crisis for Kennedy had declined significantly" after that date (p. 97).

The authors found two surprising results in their analysis of the Dominican crisis. First, they found a lack of any severe stress themes in any of Johnson's remarks during the early stages of the crisis, April 27–30. It is only with the May 2 speech, after the original wave of American troops had landed and additional troops moved in, that themes begin to register at the extreme stress level. Furthermore, the authors found that the most stressful theme during the crisis was neither concern over a Communist takeover nor concern over the introduction of American troops, but rather concern for U.S. civilians.

The analysis of the Cambodian crisis offers additional surprises. The most stressful themes were the U.S. relationship to Cambodia and the South Vietnamese role in the operation. The most surprising finding was the lack of stress associated with the theme of domestic turmoil—i.e., Kent State and its aftermath.

Although the book is, in my opinion, well worth reading, it does have some major flaws. The authors maintain that they have developed a psycholinguistic profile for the three presidents during international crises. Therefore, they argue, "if analysts find deviations from one or more of these characteristics, they should be alerted to the possibility that some type of psychological dysfunction has probably taken or is taking place" (p. 165). How can one construct a "normal" profile on the basis of one case? Additional data from other crises during the same administration are needed to make a secure claim of a valid psycholinguistic profile.

This leads to a related point. If inferences are to be made on the basis of changes in psycholinguistic posture, one must be able to ascertain what the "normal"—i.e., noncrisis—psycholinguistic pattern is for each president. Thus another major flaw in the book is the lack of a control group of noncrisis data. The authors explain the lack of such data by stating that "noncrisis video and audio tapes are made infrequently, and they are not kept for long" (p. 18). Having done research at both the Kennedy and Johnson libraries, I know this statement to be inaccurate. A quick call to the two presidential libraries confirmed the existence of audio tapes of presidential press conferences and such noncrisis speeches as State of the Union addresses that could have been used as a control group. Although similar data on Nixon would have been more difficult to gain access to, a call to the CBS News archives confirmed that such tapes on Nixon are available there.

Another flaw in the book is in its interpretation of data. At one point, the authors attempt to explain the differing patterns of fluctuation in levels of stress over time. Their explanation of why the pattern in the Dominican crisis is different from the other two is that the other two were "embedded" crises, meaning they were embedded in a larger issue. The Berlin crisis was embedded in the Cold War and Cambodia was embedded in Vietnam. In contrast, according to the authors, "the Dominican crisis is a discrete event for President Johnson" (p. 140). This explanation seems to ignore that it was exactly during the Dominican crisis period that what many analysts have called the major decisions on the escalation of the Vietnam war were made. Therefore, anyone looking at that period as a whole would be hard-pressed to argue that the two events could be psychologically separated in Johnson's mind.

Finally, the issue of the extent to which voice stress analysis is useful in explaining crisis behavior must be discussed. The authors are the first to admit that voice stress analysis has its limitations, the greatest of which is that the stress recorded by the machine can be caused by "deception, uncertainty, centrality to desires or anticipation" (p. 12). Thus, voice stress analysis tells us what themes, if any, registered psychological discomfort. It does not and cannot tell us what caused the discomfort.

For example, one of the major findings in the book is the high stress associated with the theme of protection of U.S. civilians during the Dominican crisis. The authors explain the cause of this stress in terms of Johnson as "a leader whose major political interests were in the domestic realm" (p. 114). However, it is equally plausible to argue that the stress was caused by the centrality of the theme to Johnson's paternalism, or that it was caused by an attempt to convince his audience that concern for the safety of U.S. citizens in the Dominican Republic was indeed a major reason for his decision to intervene. A third alternative explanation is that the stress was caused by Johnson's attempt to deceive his audience as to the importance of the issue. The point is that voice stress analysis cannot, on its own, explain stress, and thus must be used in conjunction with other methods, like research on presidential belief systems, in order to explain crisis behavior adequately. To their credit, the authors readily admit this.

When judging this book, one must keep in mind that explaining foreign policy crisis behavior is like putting together a 1000 piece jigsaw puzzle. Wiegele and associates have given us a new and tantalizing piece to be fitted into the puzzle. This book does not explain international crisis behavior. It does, however, give us an additional tool with which to explain crisis behavior. On that basis, it is well worth reading.

J. PHILIPP ROSENBERG

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Ripe for Resolution: Conflict and Intervention in Africa. By I. William Zartman. (New York: Oxford University Press, for the Council on Foreign Relations, 1985. Pp. x + 260. $24.95.)

Twenty-five years after the "year of Africa"—the proliferation of new states in 1960—the most marginalized and divided of all the continents is once again the focus of international attentions. The "discovery" of Ethiopia's drought and South Africa's struggle by Doonesbury, Live Aid, and Artists United Against Apartheid is evidence of the unlikelihood of foreign actors ever getting "Out of Africa" for very long. Contrarily, just when the new international division of labour has diminished external economic interests in Africa, the new Cold War has revived extraneous strategic issues. William Zartman's comprehensive and catholic case studies of the latter largely exclude the former. This magisterial mix of his two fortes—negotiation and Africa—come together well in this major study, which is both pragmatic and powerful. Yet Zartman's aversion to political economy lends a certain unreality and superficiality: Africa's developmental disasters, from droughts to the encroaching of the desert to indebtedness, hardly intrude as the backdrop to the dramas of diplomacy and strategy. The continent will be forever ripe for resolution so long as it remains starved of development.

Zartman's informed investigation of interstate discussions and coalitions is thus diminished in its salience and relevance by its failure to treat the economics as well as politics of underdevelopment: What are the social as well as strategic bases of conflict? Can protracted negotiation change patterns of inequality either between or within states? Would diplomatic interventions be less necessary if economic expansion had not been so shortlived in the first flush of independence?

Zartman's purpose in this always stylish and sometimes humorous monograph (e.g., "The more it develops, the more Zaire—now a vacuum of attraction—will become a major pole of attraction and reaction in central African relations" [p. 126]) is to describe, compare and evaluate patterns of African and American involvement in four crucial and continuing regional conflicts: the Sahara, the Horn, Shaba, and Namibia. His historical, analytical, and linguistic skills are best displayed in the discussion of the Sahara, but all four are convincing, comprehensive, and cohesive; the author's well-established Africanist competence is employed to good effect.

The shorter introduction and explication/prescription are neither as sustained nor as satisfactory, despite being based on Zartman's well-regarded negotiation investigations. Instead, early definitions of and distinctions among crises and conflicts (pp. 8–15)—how local?—are compounded in the end by belated typologies of patterns, dimensions, and phases (pp. 228–38). The juxtaposition of African and American interests is sustained throughout, yet the reconciliation of "Africanist" and "globalist" forces in successive U.S. regimes cannot be so readily reconciled: "America needs—and Africa deserves—a policy that recognizes the importance of Africa to the United States within the context of its global obligations" (p. 244). The logic of super- and great-power antagonisms compounds intra- and extra-African differences in complex and changeable coalitions—*renversement des alliances* (pp. 30 and 241)—and America's greater "interest in conflict resolution than in seeing one side win" (p. 220) is surely a subtle statement of preference for the status quo.

Zartman concludes by asserting that "it is in both American and African interests to provide assistance in regional peacekeeping and peacemaking functions now" (p. 251). Yet throughout, the prescriptions are tactical (p. 250) rather than structural, and the costs of containment miniscule compared with the price of development. The social bases of conflict cannot be forever constrained by negotiation, no matter how professional.

Nevertheless, the limitations (no political economy) and contradictions (globalist and/or Africanist preferences) of Zartman's framework, most apparent in the 20% of the volume's endpieces concerned with context and contrast, do not seriously detract from the utility of his excellent case studies. In these definitive analyses of continuing regional conflicts, Zartman recognizes a variety of actors, interests, periods, and connections, and he relates geography to strategy, and personality to process. He also touches on history—e.g., the alternative, antagonistic Ethiopian (p. 77) and Somali (pp. 74–76) "empires"—economy, and ecology, as well as on some remarkable "indigenous" defensive measures such as Morocco's desert wall (pp. 39-40) and South Africa's Nkomati Accord (pp. 200-201). Yet the state- and Americo-centric perspective is present throughout, tending to exclude either socialist powers or proletarian forces.

Aside from the juxtaposition of continent and context, the major contribution of Zartman's treatise surely lies in the notion of the "ripe moment" (pp. 9 and 220):

> Moments, when ripe, do not fall into one's hands; they have to be taken with skill. . . . Thus, for the conciliating power, it is a question not only of correctly identifying the right times to move but also of moving the times with skill. (p. 237)

However, such conjunctures are quite rare (e.g., they occur in Zimbabwe but not yet in Namibia), and reflect economy and polity as well as diplomacy and strategy.

Moreover, although the author adopts a common structure for each substantive chapter—from background and conflict to conflict resolution—he fails to contemplate whether the Sahel and Shaba, and the Horn and Namibia really are comparable in terms of history, intensity, centrality, or sustainability. In particular, the potential regional military-industrial complex of South Africa may put it into a distinct category in terms of destructive potential. Furthermore, the complexities of such relatively industrialized or urbanized political economies inevitably generate internal differences and disagreements over foreign relations—e.g., the *verligte-verkrampte* standoff in South Africa (p. 205) or the *mestiços*—black fight in Angola (p. 194). Thus, there could be reasons other than ripeness for the successful resolution of the antisettler struggle in Zimbabwe—e.g., decolonization—compared with continuing tension on the Horn—e.g., different definitions of state, nation, and territory.

Despite the historical insights and tactical aspects, Zartman's policy proposals are more modest than his analytic frameworks. Although negotiation skills can be enhanced, he concludes that "only time resolves conflicts, but time needs some help" (p. 237). Yet, moment, or conjuncture, is not defined by diplomacy or strategy alone; it can also be determined by ecology and economy. Zartman occasionally recognizes such structural constraints or determinants—e.g., the relentless encroaching of the desert in Mauritania (p. 35) and the Horn (p. 91), or the low cost of South Africa's in-depth defense of Namibia (p. 166). Yet the six causes of continental conflicts clearly have social as well as strategic roots, such as "leftover liberation movements" (pp. 12–16). Already the real cost for South Africa of regional destabilization and national repression is greater than the author calculates. Thus, Zartman's welcome sense of changing time and place in terms of interstate negotiation fails to extend to intrastate contradiction. Nonetheless, this is an important if not seminal contribution to both African and conflict studies, supported by wide-ranging sources and attractive maps. The continent may yet be ripe for a conceptual revolution.

TIMOTHY M. SHAW

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POLITICAL ECONOMY

Forecasting Political Events: The Future of Hong Kong. By Bruce Bueno de Mesquita, David Newman, and Alvin Rabushka. (New Haven: Yale University Press, 1985. Pp. x + 198. $22.00.)

The search for a theoretically based model that will enable analysts to make accurate forecasts of events and policies has, to date, yielded quite meager results. This stimulating and provocative book presents and applies a mathematical model that, its authors claim, enables us to make such predictions with a high degree of precision. This claim must be taken seriously, because their approach is an extension of the expected-utility theory that Bruce Bueno de Mesquita developed and tested in his important book *The War Trap* (Yale University Press, 1981).

The components of the model, the mathematics involved, and the assumptions upon which the model is based are explicitly and lucidly laid out in chapter 2. Policy choices are the outcome of conflicts between groups headed by strong leaders who act rationally. Hence, the leader of any group will select the policy with the greatest net gain. Decisions are influenced by such factors as orientation towards risk, the anticipated impact of the actions of other groups, and the perceived importance of the issue. A set of equations that incorporates these considerations enables the analyst to calculate the expected utilities of the major actors, and thus to predict the likely outcome of any policy conflict. The remainder of the book is devoted to applying the model to the case of Hong Kong. The analysis suggests that, even though the Chinese government will try to maintain the special position of Hong Kong, domestic political pressures will inexorably lead to the erosion of differences and to important policy changes that are explicitly spelled out.

The approach has strong appeal, for several reasons. The model is parsimonious, flexible, and can be used for simulations that permit the analyst to consider different assessments of any situation and to evaluate negotiating positions. Most importantly, it seems to work: it accurately predicted the outcome of the negotiations between the Chinese and the British on sovereignty, the economy, legal rights, and currency, and came very close on property leases. Moreover, the authors are sensitive to its limitations, which include the need to refine the model so that it can handle such elements as log rolling and tradeoffs.

The model did not achieve complete accuracy because "a key alternative was not identified. . . . The experts did not anticipate" (p. 133). In other words, the accuracy of the forecasts ultimately depends, as in any model, upon the reliability of the data that are utilized. Expert judgments concerning the identity, attitudes, and capabilities of major domestic and external actors are required, but specialists have often differed in their analyses of domestic political configurations, and even a consensus may be erroneous. The authors recognize but tend to minimize this problem. It remains to be seen just how serious it will turn out to be in practice.

Nor is it at all clear why the model should work, given the nature of its assumptions concerning rational behavior. A large body of theoretical and empirical literature has demonstrated that the "rational actor" model does not accurately describe how decisions are actually made in a large variety of situations. Rather, it suggests that leaders engage in "satisficing" behavior, and that bureaucratic and other factors severely constrain the "rationality" of the decision maker.

That such a theoretical conflict exists is testimony to the quality of the work reported in this book. Although persons familiar with the expected utility theory will probably find little that is new here, others will find this short volume a useful and important introduction to a new way of carrying out political analyses.

JOSEPH SZYLIOWICZ

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Paradoxes of Rationality and Cooperation: Prisoner's Dilemma and Newcomb's Problem. Edited by Richmond Campbell and Lanning Sowden. (Vancouver: The University of British Columbia Press, 1985. Pp. 366. $15.95.)

The value of a paradox is that it raises issues. Although it cannot be concluded from Richmond Campbell and Lanning Sowden's collection that Prisoner's Dilemma and Newcomb's Problem are paradoxes of the traditional sort (many contributors argue that they are not, for a variety of reasons), there can be no doubt that questions of importance and difficulty are addressed in these two problems.

This volume concentrates on philosophical questions. It focuses on the foundations of decision theory, but begins with ethics and ends with a discussion of the social contract and "tit-for-tat." Its aim is to show the development of its chosen issues, mixing reprints of "classics" with original papers that are often developments or rebuttals. The mixture is good and the objective is achieved in large measure.

The material is organized into four sections—on one-shot Prisoner's Dilemma, on Newcomb's Problem, on the connection between them, and on iterated Prisoner's Dilemma—that are preceded by a general introduction. The introductory section by Richmond Campbell provides adequate background and connecting material for the entire book.

Immediately following, articles by Lawrence H. Davis, John Watkins, David Gauthier, and Edward F. McClennan explore the nature of play in one-shot Prisoner's Dilemma, raising moral questions relating to the choice of cooperation as well as some corresponding issues in decision theory. The next section, on Newcomb's Problem, contains an introductory article by Robert Nozick, a resolution proposed by Allan Gibbard and William L. Harper, and considerable debate involving Terence Horgan, Ellery Eells, and Frank Jackson and Robert Pargetter. It concludes with Isaac Levi's broad criticism of decision theory.

A player of Prisoner's Dilemma who attaches a high probability to his opponent's matching his strategy choice faces a decision problem that is (arguably) essentially Newcomb's Problem. This situation is examined in the subsequent section by David Lewis, Lawrence H. Davis, and Jordan Howard Sobel.

The concluding segment contains discussion and analysis of various extensions of Prisoner's Dilemma, including repeated play, and more than two players. Reprints of articles by David Braybrooke, Jordan Howard Sobel, and Robert Axelrod, and a new contribution by Russell Hardin, raise questions about the nature of cooperation, the development of the social contract, and the extent to which Prisoner's Dilemma has thrown and can throw light on these questions. The need for an analysis of the development of cooperation in multiplayer Prisoner's Dilemma is a common theme.

Campbell and Sowden's book is resolutely philosophical in its material and approach. It will therefore only be of marginal interest to many political scientists (such as formal theorists) and others who use Prisoner's Dilemma as a basic modeling tool. Nonetheless, this collection is well organized and well introduced, and like a good paradox, succeeds in raising issues.

D. MARC KILGOUR

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Modeling Metropolitan Economies for Forecasting and Policy Analysis. By Matthew P. Drennan. (New York: New York University Press, 1985. Pp. xx + 242. $40.00.)

Using annual data from 1958 to 1978, Drennan develops econometric models for the New York–Northeastern New Jersey Standard Consolidated Area (SCA) and New York City (NYC). He estimates income equations for 53 industries, which is much more than the average for regional models. Such a high degree of disaggregation has a price, however. The model is recursive rather than simultaneous, with the results of the export sector equations used to drive most of the local industry income equations. National value-added-per-employee equations for each industry are developed in order to translate industry income forecasts into forecasts of industry employment.

In order to demonstrate the generality of this approach, a model of the Baltimore Standard Metropolitan Statistical Unit (SMSA) using annual data from 1958 to 1976 for 28 industries is also presented. Drennan then extends the results of the SCA and NYC models. Utilizing industry-occupation matrices developed by the New York and New Jersey State Labor Departments, the occupational breakdown for each industry is obtained. In order to make short-term forecasts, quarterly versions of the SCA and NYC models are developed for seven sectors. In addition, Drennan identifies the major sources of revenue for NYC, and develops a model of the NYC fiscal sector. Finally, the annual version of the SCA and NYC models are used to generate long-term forecasts for 1987 and 1990. The tax revenue equations of the NYC fiscal model are used to project revenues for fiscal years 1986, 1987, and 1988. When these results are compared with the expenditure forecasts in the New York City Financial Plan of January 1984, substantial deficits result.

The book begins with a brief review of regional econometric modeling and a good discussion of export base theory with an explanation of the identification of the export and local sectors of a regional economy. In describing the data, Drennan provides a summary of historic economic trends showing the decline of manufacturing and the rise of the service sector. Consequently, the book is tractable for the novice in regional modeling.

The book could be improved by including more detailed discussion of alternative model specifications. While space limitation in journal articles often precludes such detail, this should not be a problem in a book. Drennan states that "all estimated equations with serious multicollinearity were rejected" (p. 42), yet he neglects to include the condition number for each equation. Given the lack of policy handles among the explanatory variables, an objective measure of multicollinearity is desirable. This absence of potential policy handles becomes quite clear in the latter part of the book, where Drennan deals with public policy questions. While he correctly points out that local government can stimulate the economy by lowering relative costs, one certainly does not need an econometric model to arrive at this conclusion. He then estimates a new NYC model using a relevant city tax as an independent variable in each equation. One wonders why this approach was not taken originally. In summary, Drennan attempts nothing new. Nevertheless, he provides a case study of regional modeling techniques that is of use to those with an interest in these particular places.

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The Social Production of Urban Space. By M. Gottdiener. (Austin: University of Texas Press, 1985. Pp. viii + 318. $27.50.)

Gottdiener approaches urban analysis as an issue of social theory. His book will be of interest, therefore, to a larger audience than those particularly concerned with urban issues. Indeed, the heart of his argument centers on his contention that social theorists' indifference to spatial relations renders current theoretical approaches ineffective for understanding and explaining contemporary social and political change. Similarly, the lack of attention to changes in social organization leads to deterministic, superficial explanations of modern urban development by both mainstream and Marxian urban analysts. To rectify these gaps in both social theory and urban analysis, Gottdiener proposes a new paradigm, based on Henri Lefebvre's work, that focuses on the dialectical relations of state, society, economy, and space. He calls this the "production of space" perspective, and adopts a "structurationist" approach to describe how structural imperatives and human choice interact to produce contingent, rather than deterministic, outcomes in conflicts over the use of space. The normative implications of this emphasis on the social processes underlying urban development place sociospatial conflicts alongside economic struggles as necessary elements in political transformation.

Gottdiener characterizes metropolitan deconcentration trends (into "polynucleated metropolitan regions") in the United States and a concomitant "revitalization" of some central city areas as "a qualitatively new form of settlement space" indicative of profound changes in deeper social relations. The inappropriateness of "outdated" analytic paradigms based on the bounded central city for explaining these trends prompts his reassessment of urban theory. He systematically reviews "mainstream" urban theory, based on ecological models of urban development, as well as Marxian political economy, Marxian structuralism, neo-Weberianism, and models of sociospatial dialectics used by European and American social scientists. In a detailed critique, Gottdiener presents—and finds wanting—the contributions of the Chicago school of social ecologists as well as those of David Harvey, Allen Scott, and Manuel Castells, among others. To political scientists unfamilar with the nuances and subtleties of "the debate on the theory of space" that has raged since the 1970s, Gottdiener's pungent epithets will seem fascinating, sometimes amusing, elements of an arcane academic dispute. They lay the groundwork, however, for his turn to Lefebvre's theory of the social production of space, an approach Gottdiener claims more effectively integrates social and spatial change than the models reviewed. Although Lefebvre's argument is not as unfamiliar to American social scientists as Gottdiener supposes, the argument is complex enough to warrant some detail.

The author's advocacy of a new paradigm stems from limitations he sees in Marxian political economy's explanations of economic change and spatial form. In contrast to approaches associating spatial forms with specific phases of capitalist development (e.g., "the post-industrial city"), he contends that changes in urban form connote the contradictory relationship between investment in the primary circuit of industrial production and investment in the secondary circuit of real estate. Gottdiener distinguishes his analysis of these circuits from other theorists' approaches by his emphasis on the direct wealth-creating capacity of the secondary circuit, its contradictory role in capital accumulation processes, and its generation of new social cleavages and territorially based interests that reflect sociospatial conflicts rather displaced economic conflicts.

These investment circuits are seen as complementary: the secondary circuit "siphons off" capital from the primary circuit, and thus offers a means of combatting falling rates of profit and avoiding accumulation crises. In contrast to political economy perspectives, both Gottdiener and Lefebvre contend that real estate investments are a direct means of capital formation, but they argue that this complementary relationship is "essentially unhealthy." Investment in real estate, though a source of capital formation, means that less capital is available for primary production. Once invested in "the build environment," capital is not easily shifted to investment in new production activities. Furthermore, real estate investment appears to be increasingly attractive, relative to investment in the primary production circuit. Gottdiener attributes this increasing desirability to several factors, but notably to the wealth generated by seemingly unlimited transformations in the use of a land parcel. In the United States, social values and institutional arrangements protect private profits gained by these land investment activities. They also ensure that state intervention consists of indirect policy support for restructuring activities, rather than direct planning of the use of space. At the local level, in particular, they encourage local officials to facilitate real estate investment activities. With this weak state role and an essentially uncoordinated real estate investment circuit, there is a tendency towards diverting either too much or too little investment from the primary circuit. This exacerbates crisis-prone economic swings and, as Gottdiener notes, results in development processes that are not necessarily functional for capitalism or logical for capital accumulation processes.

This is an ambitious effort and a complex, compelling argument. It links uneven local development patterns and processes to global economic changes, and describes the spatial equivalent of conflicts of use and exchange values. It portends a local politics increasingly dominated by conflicts over the uses of land and by groups' efforts to renegotiate the distributional and environmental impacts of development activities. It directs political scientists' attention to an urban research agenda centered on the "politics of spatial production," particularly the actions of class fractions such as finance capital and other interests organized around land investment, and the state. Finally, it argues that, to the extent that political interests are identified with these sociospatial concerns rather than with class interests, conventional political economy approaches to urban analysis will be inadequate in addressing this new research agenda.

Whether Gottdiener's paradigm offers a useful alternative is somewhat problematic. Its explicit focus on the American case is understandable but limiting, and there is surprisingly little discussion of the extent to which this paradigm is useful in explaining non-American development patterns. Unfortunately, the final chapters discussing the political articulation of these sociospatial dynamics are not as cogently argued as the initial critique. In particular, the vague specification of the secondary circuit reduces this promising insight to a familiar emphasis on real estate and finance interests and a surprisingly narrow consideration of the local state. Gottdiener's notion of a pro-growth network is a weak derivative of the concept of a pro-growth coalition figuring in many recent analyses. It does not suggest a new or distinctive understanding of these relationships. Similarly, his pat portrayal of the local state as a virtual "marriage" of public power and private real estate interests veers away from his structurationist perspective by ignoring contingent community responses to restructuring pressures. It fails to consider the conditions under which such variations occur, and overlooks the tensions stemming from local officials' needs to accommodate electoral and economic constituencies. Finally, Gottdiener gives scant attention to political scientists' contributions to urban theory. While not necessarily an egregious omission, incorporating evidence from the many empirical analyses of sociospatial politics would strengthen his arguments and deepen our understanding of their implications for political life.

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An International Political Economy. Edited by W. Ladd Hollist and F. Lamond Tullis. (Boulder, CO: Westview Press. Pp. viii + 300. $32.85, cloth; $14.85, paper.)

An International Political Economy is the inaugural edition of a new series to be published under the auspices of the International Political Economy Section of the International Studies Association. Its avowed purpose is to explain the structure and evolution of the international economy, and to detail the economic and political impact of change on members of the system. As the editors note, we are living in a time of dramatic change, when the orthodox modes of looking at the world no longer suffice to help us make sense of the changes. The problems that rapid structural change impose are normative, empirical, and policy-political (p. 5), as well as theoretical. The series is intended to be an open forum for discussion of these problems.

This, the first volume, is an overview of the field gathering together 10 essays divided into three broad categories: (1) defining international political economy (Susan Strange, and Ladd Hollist and James Caporaso); (2) exploring the evolution of the system (Christopher Chase-Dunn, Robert Solo, and Stephen Resnick, John Sinisi, and Richard Wolff); and (3) exploring outcomes in the specific issue areas of international debt (Jonathan Aronson), industrial crisis (Robert Walters), multinational corporations (Robert Kudrle), commodity trade (Jock Finlayson and Mark Zacher), and rural development (F. LaMond Tullis).

In the first part, Susan Strange finds international political economy (IPE) to be the dubiously legitimate offspring of both international economics and international relations. Arguing that the former has concentrated on efficiency, treating politics as an exogenous variable, and the latter on peace, treating economics as a mere handmaiden to the more important task of preserving the system, Strange finds neither sufficient to address the problems of the interaction of politics and economics. While IPE carries the genes of both parents, it wholly resembles neither. She finds fruitful sources for concepts and theories in economic history, development economics, and international business. Similarly, Hollist and Caparaso, noting that "derived economic and political concepts are of disappointingly little help in understanding political-economic issues" (p. 37), argue for greater attention to economic and social history and comparative social history, along with greater attention to how the state shapes and is shaped by interaction with the market.

Both Chase-Dunn and Solo adopt historical perspectives on the evolution of the international system and the state, respectively. Both find the territorial state to be incapable of dealing with problems posed by the globalization of the economy and interdependence. Both argue that the nation-state must be transcended, either by some confederation that would make the pursuit of "collective rationality" possible (Chase-Dunn), or by some international civil service to manage the problems that have escaped from the grasp of national governments (Solo).

The most interesting and most original attempt to deal with the structural aspects of the international political economy is the rereading of Resnick, Sinisi, and Wolfe of Marxist class analysis focusing on the question of foreign exploitation. They argue that the analysis of aggregate capital transfers is an inadequate indicator of exploitation. The key to their argument is the disaggregation of capital transfers into that produced by the creation of surplus value and that which is a product of the redistribution of surplus value. Only the former qualifies as an indicator of "foreign exploitation," and it could be nil even in the face of high levels of direct transfers from the periphery to the core. Nationalism may halt foreign domination, replacing it with national exploitation or social revolution, and yet high levels of aggregate payments would still flow northward, though now in the form of distributed surplus, and by definition not part of the mechanism of exploitation. They suggest that there is no necessary relationship between capital transfers and foreign exploitation, or between nationalist revolutions and the end of exploitation and foreign dominance. If we accept the logic of their complicated theoretical analysis, then much of the theorizing about peripheral dependency rooted in transaction analysis must be revised. Conclusions about the exploitive or beneficial effects of integration into the capitalist world economy drawn from radical and orthodox analysis are suspect. Many, however, will find their narrow focus on exploitation to be of minor importance when compared with the larger questions of the impact of foreign investment on development.

The policy section contains five empirically oriented chapters detailing how policy makers have and ought to respond to the sweeping changes confronting them. Aronson, for example, examines the complex state of financial interdependence and the "wild dance on the chasm of calamity" by banks, creditor countries, debtor countries, and international institutions. Creditor countries, particularly the U.S., need to pay closer attention to the intimate links between trade, development, and finance if a collapse of the system is to be averted. Walters explores U.S. patterns of reaction to change in steel, automobiles, and semiconductors, and argues that the least costly and most likely policy alternative—protectionism—is not likely to succeed in remedying the underlying production problems. He joins others in calling for a national industrial policy to supplement trade policy. Robert Kudrle applies public choice theory to Multinational Corporations (MNCs) in an attempt to account for the differences in response to multinationals between Less Developed Countries (LDCs) and developed countries. He argues that a public choice approach helps identify issues about MNCs but little support for this argument is found in the analysis. Finlayson and Zacher analyze the negotiations over International Commodity Agreements in order to understand the divergences between normative goals of general commodity regulation and the outcomes in specific commodity regimes. Lastly, Tullis reviews the current thinking on rural development as an alternative to the urban-centered development policies of the past. He advocates extending massive credits to small-scale, labor-intensive agriculture as a way of shifting from export-oriented agriculture toward production for domestic consumption. An analysis of the political impediments to such reform movements in Latin America ends on a pessimistic note.

The yearbook is off to a good, if somewhat uneven, start. Though we can all concur that neither politics nor economics is sufficient to address many problems, the definitional section too easily overlooks the contributions of rational choice models and the literature on regime formation and maintenance, and gives scant attention to hegemonic theory. Much of the literature that the authors suggest needs to be incorporated into the field has already been incorporated by way of world-systems analysis and dependency analysis. Furthermore, many of the pieces seem compilations of existing material, with only a few (e.g., Resnick et al., Kudrle, and Tullis) bringing new theoretical insights to bear on the literature. However, these are minor qualms, and the editors indicate that they will be addressed in future volumes.

This IPE sampler stands as an interesting introduction to the field—more useful for novices than old hands. Yearbooks and annuals in the social sciences have come and gone, and few have left any lasting mark on their respective fields. With vigorous editorship and the selection of material that both challenges and informs, this one could break the cycle.

DON HAYNES

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The Limitation of Conflict: A Theory of Bargaining and Negotiation. By L. N. Rangarajan. (New York: St. Martin's Press, 1985. Pp. 327. $32.50.)

In this volume, L. N. Rangarajan, Ambassador of India to Tunisia, attempts to construct a theory of bargaining based in large part on his experiences in multilateral negotiations. While it is refreshing to read a theoretical work written by a practitioner, the book fails to provide many useful new insights.

What Ambassador Rangarajan puts forth as a "theory" is in fact a series of loosely connected essays on various aspects of bargaining and negotiation. To his credit, the author addresses topics not usually covered in the standard texts on this subject, such as the importance of time and memory in the negotiating process. Unfortunately, so many areas are covered that the author is largely unable to devote sufficient attention to any one of them. As a result, an uninitiated reader will be pumped full of information and anecdotal evidence on a variety of subjects, but will likely reach the end of the book without a clear idea of how it all fits together.

Ambassador Rangarajan's desire for a new approach to the study of bargaining is based, in part, on his belief that game theory is "inadequate" for the purposes of analyzing the topic (p. 7). However, his objections—that the theory is "static" (p. 50) and "concerned only with those negotiations which are assumed, *a priori*, to end in agreement" (p. 7)—simply do not hold water. Indeed, recent work in the field on the topics of sequential bargaining and sequential equilibria answer many of the questions raised by the author in a much more rigorous, if somewhat less widely understood, fashion.

It is perhaps the case that the author's confusion on the usefulness of game theory results from his desire for unique solutions to bargaining problems. As an example of the difficulty of applying game theory to *n*-person negotiating situations, Ambassador Rangarajan puts forth a game in which three men (A, B, and C) are asked to divide one unit of currency between then, the division to be determined by majority vote. As in the related game of Couples, the imputation (1/2, 1/2, 0) can be countered by player C with (0, 2/3, 1/3), which can be countered by player A with (1/2, 0, 1/2), etc., and a unique equilibrium does not exist. The author concludes the description of this example by observing that "having fallen into the trap of circular reasoning, game theory cannot find a rational and optimal strategy for the three players" (p. 144). Of course, that this situation does not have a unique equilibrium should not lead anyone to the conclusion that game theory is useless. Indeed, what the author seems to ignore is that many games, and many "real-life" bargaining situations, have many possible outcomes.

To confuse matters even more, the ambassador's solution to the game is an equal division of the unit of currency—"may be giving the odd left-over penny to the first who propounds this unique solution!" (p. 145). This agreement will be arrived at because it satisfies the condition of "equality of dissatisfaction" among the players—a solution concept invented by the author. However, this imputation, (1/3, 1/3, 1/3), is dominated by any number of other divisions, and hence, cannot be included in the set of possible solutions. The ambassador's solution obtains only if all parties desire to be "fair" or place a very high value on reaching a unanimous agreement. Rarely do the participants in negotiations have these preferences.

It is also disconcerting that the author devotes a chapter to coalition formation, size, and disintegration without citing Riker (*The Theory of Political Coalitions*, Yale University Press, 1962), and a chapter to bargaining power that addresses weighted voting but ignores the Shapley power index. Indeed, the gaps in Ambassador Rangarajan's coverage of existent works on bargaining are so large that I cannot recommend this book to advanced undergraduate or graduate students.

Finally, it should be noted that the final chapter, "World Order and Conflicts: The Next Fifteen Years," is largely unrelated to the rest of the book. Instead it contains the Ambassador's personal assessment of the current international environment. Unfortunately, these observations are limited to the bashing of Ronald Reagan, Margaret Thatcher, *The Economist*, and proponents of stable nuclear deterrence. It should go without saying that writing such as this is better suited for the editorial pages than an academic publication.

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Social Science and Social Policy. Edited by R. Lance Shotland and Melvin M. Mark. (Beverly Hills, CA: Sage Publications, 1985. Pp. 375. $29.95.)

There is a bit of irony in this excellent book. It is dedicated to the memory of Carolyn Wood Sherif, "who was firmly committed to the use of the social sciences to improve the human condition and to further social justice," yet one comes away from the book feeling that Harold Lasswell's vision for the "policy sciences," in which researchers and practitioners work together to improve public programs, is still a long way off and may never be reached.

The principal message of most of the 19 authors is that the role of social science is primarily technical: it cannot and should not enter into value questions. For example, in their chapter on gun control, Rossi and Wright contend,

> A society in which social scientists play crucial policy roles through their research is a society in which human values have been subordinated to technical considerations. . . . In a truly democratic society, social science must be content with an advisory but not dominating role. (p. 331).

However, the book also takes some steps away from this strictly positivist position concerning facts and values. In the opening chapter of part 1, which deals with general issues, Thomas D. Cook argues that positivism is dead and should be replaced by "multiplism," by which he means multiple operationalism and multimethod research. Cook rejects determinism, belief in perfect prediction, attempts to reach generalizations, belief in value neutralism, and insistence on operationalism. He does retain the positivist assumption that there is a real world that we can know to a useful, though imperfect, degree through observation.

The remaining chapters in part 1 point out obstacles to ideal research. Sechrest notes that almost all of the data on which we depend are biased and distorted, leading to the "garbage in-garbage out" syndrome. McLaughlin writes that because implementation choices dominate outcomes, and implementors pursue multiple and often competing goals, it is impossible to prove a program works as we do a principle of physics. Pettigrew says that social science research is filtered through the news media, and that this leads to great distortion, since the media do not know how to cover social service.

Part 2 of the book focuses on policy areas. Here again, the authors give social science a narrow, technical role. For example, Taylor argues that social science research cannot resolve the difficult issues in affirmative action: "Who should lose, and who should gain, and who should shoulder the burden of righting society's wrongs—these questions are philosophical, not scientific" (p. 139). Paula Cain's conclusions about comparable worth in the next chapter are similar; real progress in this area was not due to breakthroughs in scientific knowledge (although the development of job evaluation systems helped); it was due to judicial interpretation of an obscure amendment to Title VII of the Civil Rights Act of 1964. The final chapter in this section by Henry Levin reaches identical conclusions: Social scientists cannot answer the question of what the level of unemployment should be, only questions about the causes of unemployment (and these not very well). This kind of knowledge, he says, is "quite marginal to the political process that determines normative reality and political strategy" (p. 173).

In the next section of Part 2, St. Pierre, who was involved in evaluations of follow-through and compensatory education, describes the controversies and misinterpretations these evaluations generated. The follow-through evaluation, St. Pierre found, was faulty and misinterpreted, but compensatory education evaluations were used by Congress because the evaluators took the time and effort to answer the questions of interest to Congress. Still, most evaluations are not used because the programs they study do not have big impacts.

Crain and Carsrud are much harder than St. Pierre on social science in their discussion of desegregation. Researchers in this area did not focus on the questions that would be helpful to policy makers—questions pertaining to the best ways of implementing desegregation—but instead looked at whether or not desegregation helped blacks. Why? Because academic institutions do not value applied research. As a result, policy makers have one set of questions and evaluators answer a different set. In the last chapter in this section, Jay Belsky argues that although there is considerable policy-relevant information available about day care, "day care policy seems to develop out of a political process that has little if anything to do with scientific inquiry" (p. 240).

In the final section of Part 2, Charles Tittle looks at research about deterrence and concludes that we know almost nothing about it, even though a good deal of research has been done:

> There is now no compelling reason to believe that social science-based policies concerning sanctions would be more efficient, useful, or morally justifiable than those that emerge from ordinary policy debate. (p. 290)

In the same vein, Jeanne C. Marsh finds that rape legislation in Michigan was helpful, but that social science research did not have much to do with it because decisions in the policy process are influenced by interest groups and events, and only minimally by relevant knowledge. As I noted above, in the final chapter on this section Rossi and Wright reach the same conclusion: Even in the best of circumstances, social science's ultimate impact on policy formation will typically be modest, and this is all it can do.

One of the principal reasons why social science plays such a limited role in policy, according to many of the book's contributors (none of whom is a political scientist), is that political factors are far more important than knowledge. The authors are psychologists and sociologists. If politics are so important, then perhaps political scientists should become more heavily involved in policy analysis. Perhaps this would lead to a more effective role for social science knowledge, since political scientists understand more about the political conditions under which social science knowledge might become more useful. In any event, this is an excellent book that would be a useful text in courses on public policy evaluation.

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Taxes, Loans, and Inflation: How the Nation's Wealth Becomes Misallocated. By C. Eugene Steuerle. (Washington, D.C.: The Brookings Institution, 1985. Pp. xii + 205. $26.95, cloth; $9.95, paper.)

This book is a very detailed study of the uneven tax and market treatment of capital income produced by the tax system in the United States. It specifically emphasizes the distorting effects of the tax system on capital income flows and loan markets, and the resulting disparity between taxable capital and labor income.

The book accomplishes four separate tasks, which are intertwined in the chapters. The first, in the best Brookings Institution tradition, provides carefully calculated estimates of: capital income from individuals and corporations; the amount of capital income that is taxable, and the effective tax rates broken down for individuals by income groups and for corporations by type of investment. Although tax experts have discussed at length the differential tax treatment of capital income versus labor income and the uneven tax treatment between types of capital investments and sectors, Steuerle provides the best and most comprehensive data to date on these subjects. This section of the book is a gold mine for opponents of the current income tax system. The author's data provide unqualified critiques of the current system based on grounds of both equity and efficiency.

The second part of the book, which will be new to most political science readers and many public finance specialists, provides a fascinating tour of the world of tax and financial arbitrage. Arbitrage is based on the ability to make financial gains by borrowing to acquire tax-preferred assets. The leverage that makes this possible is the tax-exempt status of some assets (e.g., municipal bonds) combined with the deductibility of interest payments on borrowed money. My more highly paid colleagues may recover the purchase price of this book many times over by putting into practice Steuerle's detailed description of arbitrage in chapter 5. Perhaps most interesting is the prospect he opens up in chapter 6 by outlining in theory a "reverse arbitrage" procedure, whereby futures markets in financial assets would allow those in lower tax brackets to borrow to purchase taxable assets, and still realize after-tax gains. Thus our tax system has realized yet another perverse equality by opening up the world of arbitrage to the poor and middle-class as well as the rich.

The third objective of the book is to assess the implication of the tax treatment of capital income, with special emphasis on the effects during periods of high inflation. The major implication, according to Steuerle, is that the tax system is separating a true measure of economic income from taxable income, financial income (as reported on financial statements), and cash flow accounting. This leads to misallocations of capital, distorted and ineffective incentives for savings, and artificial changes in organizational structures (mergers, divestitures, etc.). In a separate chapter, Steuerle convincingly argues that these distortions are enhanced by inflation. In addition, because most macroeconomic models assume "neutrality" of inflation and inconsequential effects of the tax system on capital markets, macro policy is also adversely affected, and must try to cope with constant instability in financial markets.

The final, and weakest, section of the book briefly outlines the theoretical effects of various tax reform proposals on problems of capital taxation. Steuerle reviews only the most straightforward efficiency effects of base-broadening combined with rate reduction, a flat tax, indexing proposals, and a comprehensive expenditure tax. Based on the reduced distortion that would result from each, Steuerle seems to applaud all these efforts, despite glaring controversies that arise when other criteria are introduced in the debates over these proposals. Because a separate book or series of books might be required to compare these approaches to tax reform adequately, his omissions are understandable. However, it does leave the reader with a sense that, for example, a base-broadening approach is roughly equivalent to an expenditure tax as a solution. Although this may be true for problems of financial market distortion, there are major equity issues between an approach that treats all income as taxable and one that exempts all capital income.

This book is essential reading for political scientists involved explicitly in tax policy and politics. I would also strongly recommend the book for those generally interested in political economy. For the latter group, the effort required to read and comprehend the book will be repaid through acquisition of a detailed understanding of real-world capital markets. For that group of political economists whose forte is "economist baiting," Steuerle's work provides ammunition both by exposing the simplicity of capital market assumptions incorporated in economic models, and by further shattering images of efficient market flows—in this case the flow of capital.

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The Politics of Industrial Change: Railway Policy in North America. By R. Kent Weaver. (Washington, D.C.: The Brookings Institution, 1985. Pp. vii + 291. $30.00.)

Industrial policy will undoubtedly reemerge as a political and economic issue in the next U.S. presidential campaign, just as it will continue to be debated within the Canadian parliamentary system. Those who seek a deliberative and balanced cross-national assessment of how public enterprise has been, and might be, pursued in the United States and Canada as an instrument of industrial policy will find much that is worthwhile in this study. Rooted in his 1982 doctoral dissertation, Weaver has generated an interdisciplinary work of policy analysis sensitive to economic, political, and public administrative forces.

Why did Weaver select railroads as an object of analysis in the domain of industrial policy? He indicates that any study of government's role in facilitating industrial adjustment must consider methods and tools government can use to intervene in specific industries. There are many instruments of intervention, but public enterprise via nationalization may be the most far-reaching and controversial tool available to government. The U.S. government has been historically reluctant to use nationalization in addressing problems of domestic industrial adjustment. However, major recent exceptions are Conrail, a consolidation of bankrupt northeastern freight-dominated railroads, and Amtrak, a nationwide intercity passenger rail service firm. Both Amtrak and Conrail exist as U.S. versions of public enterprise. Canada has a long tradition of use of public enterprise corporations, and has proliferated a great many Crown corporations. The Canadian National Railway, which competes throughout Canada with private freight-hauling railroads, and the newer VIA Rail Canada national passenger service firm are both public enterprise institutions. These four railroads represent the major empirical data base for the study.

A paramount assumption of the work is that "the industrial adjustment problem should not be viewed primarily as a trade problem" (p. 2). Weaver argues that many industries that are not subject to fierce international competition —railroads, for example—"face the same adjustment dilemmas as trade-threatened industries and will experience many of the same protectionist outcomes." Therefore, the rail sector affords, according to Weaver, an excellent arena for comparative examination of industrial adjustment policy and public enterprise in the United States and Canada. He concedes that "the reader must be convinced that the case is not idiosyncratic but generally applicable" (p. 27). This reviewer remains somewhat skeptical. The railway case has little in common with the plight of current North American steel, textile, and auto manufacturers. A central stimulus to debate about industrial policy has been concern about failing industries threatened by foreign competition. The decline of U.S. and Canadian railroads can be attributed to many factors (competing modes of transport, high labor costs, huge maintenance and tax costs, etc.) but not to foreign competition.

Nevertheless, Weaver's thorough analysis of the problems of U.S. and Canadian railroad public enterprise operation generates a sufficient body of theory, generalizations, and testable propositions. The cross-national comparison is so sound that this reviewer found significant merit in the study, even if the case is rather idiosyncratic.

In addressing types of industrial policies, Weaver presents market-oriented policies geared toward general macroeconomic and social policies and triggered by market signals (unemployment compensation, education and training programs, monetary policy credit controls, etc.), protectionist policies intended to provide sector-specific avoidance of concentrated costs for any domestic group (tariffs, import quotas, regulation), and accelerationist policies designed to speed up transitions when political markets overwhelm economic markets, or when private markets appear unlikely to produce efficient outcomes. Weaver seems most enthusiastic about use of accelerationist policy. He indicates that the policy that may be most appropriate for a given set of circumstances must take into account whether the firms or sectors addressed are occupied by sunrise, mature, or declining industries. Railroads in North America are a declining industry that, according to accelerationist policy, must reduce excess capacity, promote exit of inefficient firms, and restructure the industry to be more competitive.

Weaver concludes that the major limitations in formulating, legitimating, and implementing industrial policy are domestic and political. Variations in political structure exist from one country to another, such that it makes little sense to pattern U.S. or Canadian industrial policy after Japanese or Western European models. In spite of its industry-specific focus, the analysis and findings of Weaver's study place it among more general economic analyses of industrial policy by Robert Reich, Ira Magaziner, Lester Thurow, and Amitai Etzioni. The political and public administrative environment profoundly affects both the management and economic viability of public enterprise.

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The Economic Institutions of Capitalism: Firms, Markets, Relational Contracting. By Oliver E. Williamson. (New York: Free Press, 1985. Pp. 450. $27.95.)

In 1937 economist R. H. Coase published "The Nature of the Firm," a short paper that, like much of Coase's work, has had enormous implications. Typically, Coase took nothing for granted and reinvestigated fundamental questions: Why are there multifunctional firms? Why should not markets prevail in every activity? Why, for example, does a large company like General Motors produce internally some things that it employs in automobile production and acquire still others in market transactions? In attempting to answer these questions, Coase invented a discipline—law and economics—and a set of concepts that are as applicable to the arena of political organization as they are to the activities of economic life. Administrative organization and markets were now viewed as alternative means to attain goals. The most important concept employed in the new discipline was transaction costs—the costs incurred in planning, negotiating, contracting for, and policing tasks. Coase's theoretical answer to the central question posed was that the comparative transaction costs at the margin will determine whether a firm chooses the market or internal organization to fulfill a task.

Like much ground-breaking work, Coase's triggered an enormous body of literature designed to clarify and amplify the original insights. The most important problem Coase raised but did not answer is the central focus of Oliver E. Williamson's brilliant new book: "Unless the factors responsible for transaction cost differences could be identified, the reasons for organizing some transactions one way and other transactions another would necessarily remain obscure" (p. 4). Williamson attempts to resolve the difficulty by providing first an elaborate theoretical framework within which the concept of transaction costs is central, but with which a considerable body of organization theory, law, and economics has been integrated. Second, the theoretical framework has been applied to a large number of particular issues such as vertical integration, the organization of work, the structure of corporations, and the problem of natural monopoly.

Before proceeding further I must address a preliminary question. Why should political scientists read a book that is mostly concerned with discussions of such economic or management topics as backward vertical integration and the advantages of the M-form corporate structure compared to the U- and H-forms? The simple answer is that the law and economics framework provides as powerful a tool for examining political organizations as it does for examining economic organizations. Indeed, the leading journals in the law and economics discipline have published papers that have examined the U.S. Senate and administrative agencies as "firms." Using such concepts as transaction costs, information costs, and incentives, it is possible to gain considerable insight into the behavior of political organizations. Bargaining and logrolling, for example, are types of transactions in which legislators and others incur costs and about which we can spell out incentives. For political scientists whose primary focus is on public policies, the dichotomy between market transactions and internal governance has an obvious parallel in the dichotomy between government regulation and market mechanisms as alternative means to achieving certain results.

Williamson explicitly disclaims that transaction cost arguments sweep all others away. Rather, they are a powerful tool capable of generating considerable insight, and are often used most effectively in conjunction with other approaches. Nevertheless, the book's fundamental focus is on transaction costs, and—the ecumenical assertion notwithstanding—other approaches, when they are considered, are usually compared adversely to law and economics. However, the comparisons are almost always made to alternative economic approaches to human behavior that are based upon behavioral assumptions similar (although not identical) to those Williamson makes. Thus, Williamson explicates two important behavioral assumptions: bounded rationality and opportunism. *Bounded rationality*, taken from Herbert Simon, assumes a maximizing posture. However, because all of the relevant costs are not known, since information is costly, one undertaking a course of action incorporates risk and uncertainty. In contrast, neoclassical economics assumes that all pertinent costs are recognized. The second behavioral assumption, *opportunism*, is "self-interest seeking with guile. This includes but is scarcely limited to more blatant forms, such as lying, stealing and cheating" (p. 47).

It is here that Williamson's model, like so many in economics, manifests its limitations. To take one extreme case, transaction cost economics is inappropriate for studying the behavior of the insane, since the two behavioral assumptions do not apply. This raises the interesting problem of delineating the political realms in which transaction cost economics is useful alone, useful in conjunction with other approaches (for example, Marxism), and not useful at all. Is transaction cost economics useful in understanding the destructive (and, indeed, self-destructive) inclinations of regimes as diverse as Stalin's Russia, Hitler's Germany, Pol Pot's Cambodia, or Qadaffi's Libya? I suspect that Freud, Le Bon or the social anthropologists provide more powerful insights to understanding such important political phenomena.

I do not wish to take anything away from Williamson's remarkable achievement when I point out that the political applications of transaction cost economics are apt to be most fruitful in constitutional settings of the type that prevail in the Western democracies and Japan. Costs are imposed upon business firms in such settings because the legal and political structures explicate and enforce rules defining loss and gain. Safeway does not literally bomb Kroger when it loses clientele to the latter, as it theoretically could in an unconstitutional regime. Bounded rationality is virtually compelled because costs can reasonably be enforced. The same dynamic applies to the House of Representatives or the Federal Communications Commission. To that extent, the second behavioral assumption—opportunism—must be modified. Bounded opportunism would be more appropriate.

When we recognize the settings in which transaction cost economics can be most fruitfully applied, and modify the second behavioral assumption, Williamson has provided a powerful mode of analysis that I hope will be more frequently applied to politics than it has been in the past. This is an extremely important book, both for what it says and for what it suggests.

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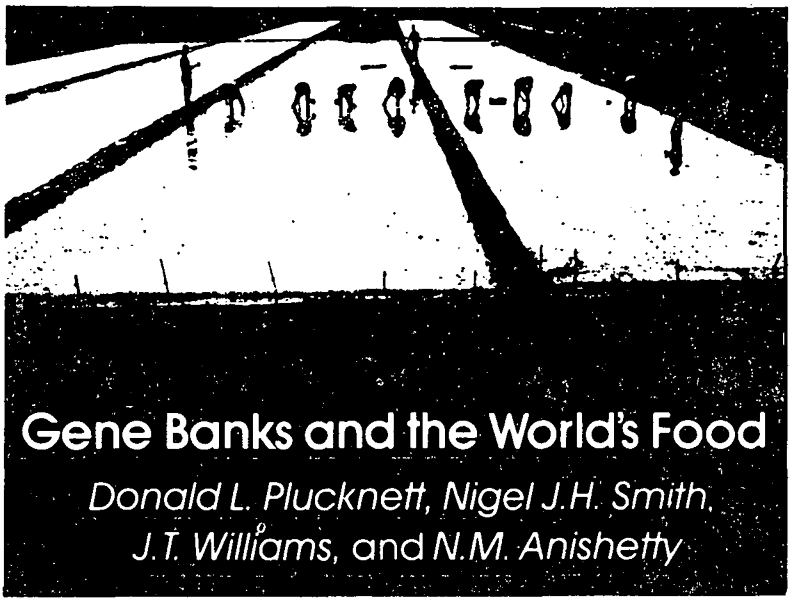

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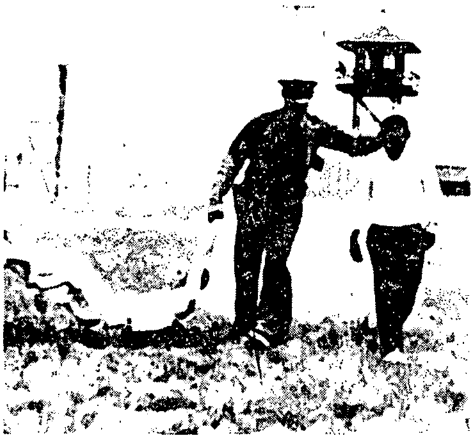

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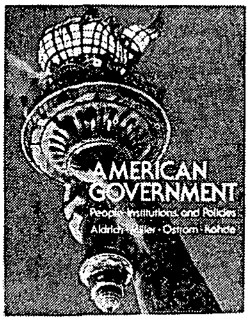
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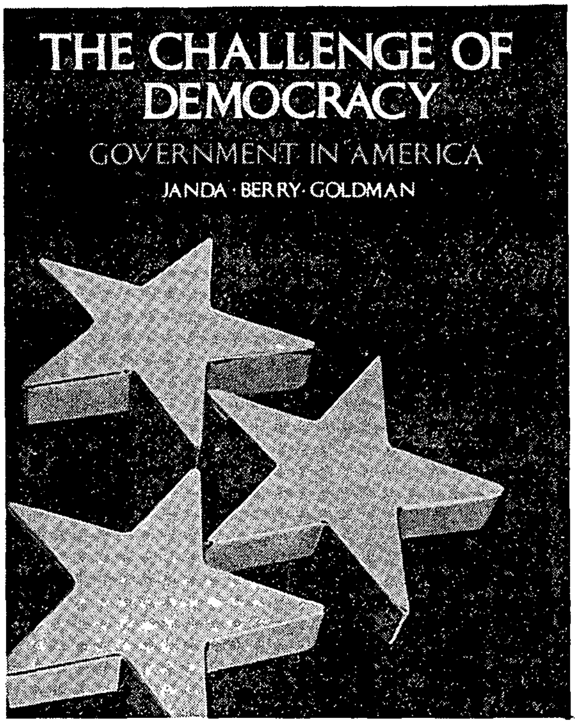

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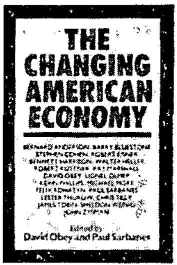
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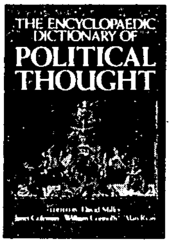

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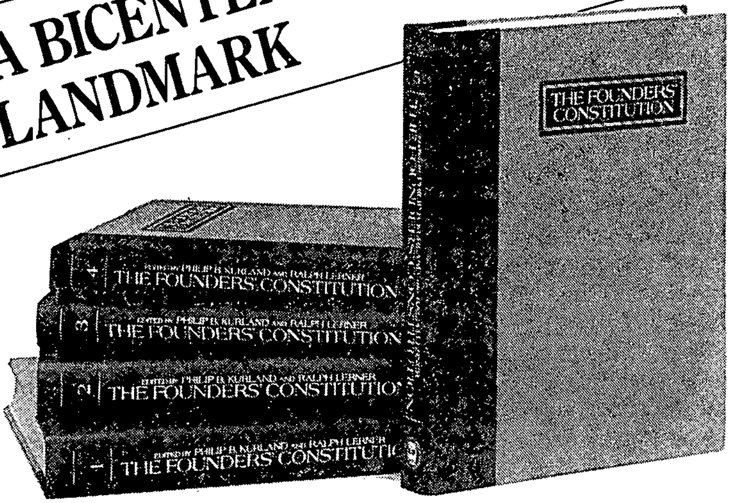
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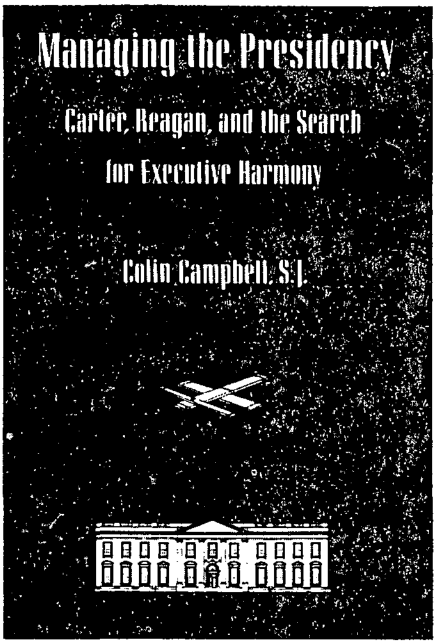

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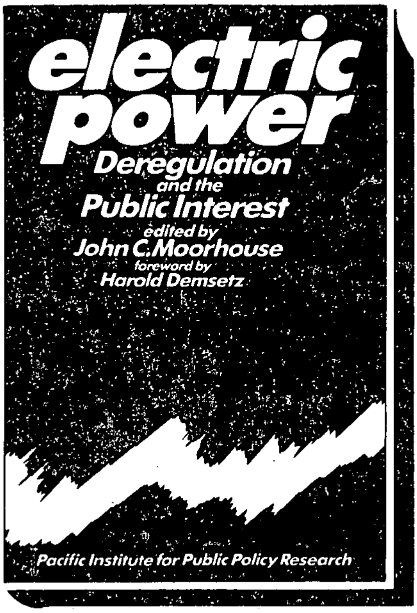
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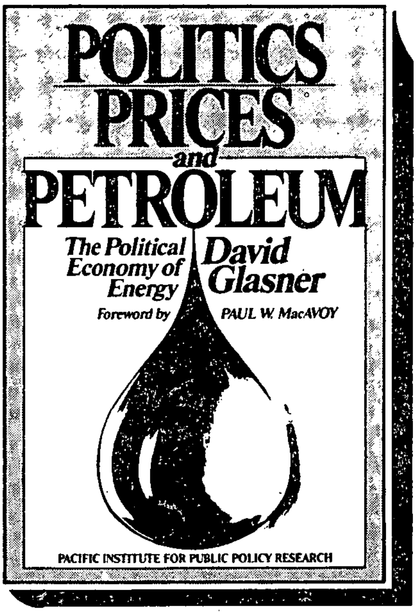
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